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GENERAL SECTION

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PART 1

VOLUME XV JULY, 1924

THE BRITISH
JOURNAL OF PSYCHOLOGY
GENERAL SECTION

A STUDY IN PREPERCEPTION

BY CHARLES FOX.

I. *The meaning of preperception* (pp. 1–3).
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I. THE MEANING OF PREPERCEPTION.

IN summarizing the results of previous experimental investigations on perception in order to discover the nature of attention Prof. William James came to the conclusion that three factors were involved; namely, the organic process of adjusting the sense organs, ideational preparation, and inhibition of irrelevant movements and ideas[1]. He considered that, in voluntary attention, the process of inhibition was merely an incidental feature and not an essential part of the process. It is evident also that conscious inhibition presupposes attention and, therefore, cannot, without circular reasoning, be used to account for it. The accommodation of the sense organs, although necessary for sensorial attention and probably always present in ideational activity, is a matter for physiology and need concern us no further. In any case, such adaptation is only a favourable condition of attention. We are left, then, with ideational preparation or pre-perception as the distinguishing feature of the attentive process. It is necessary, however, to avoid what Prof. James Ward calls the doctrine of presentationism, according to which presentations are regarded as entities interacting amongst themselves; and to do so we must keep clearly before us the fact that the interaction of presentations is always dependent ultimately on attention[1]. The mistake here alluded to is especially prevalent amongst experimentalists who are apt to confuse the conditions favourable to attention with the attentive process itself. Such process involves the conception of mental activity, which is evident whenever a conscious state is the result of previous conscious processes. It is safe to say that all attempts to dispense with the notion of mental activity on the basis of experimental researches have so far completely failed.

---

[1] *Principles of Psychology*, vol. I, ch. XI.

In his valuable, but strangely neglected work called *Problems of Life and Mind*[2], G. H. Lewes first used the term preperception. He pointed out that the effect of previous experience was to enlarge our present perceptions making us more and more independent of the immediate stimulus, more and more masters of the external world. The present is largely the resultant of the past revived as a present experience, "and this revival makes *pre*-perception a factor in perception." In the same way a new idea "must be prepared for, *pre*-conceived by the exhibition of its points of similarity and attachment with familiar conceptions." Both a new object presented to sense and a new idea presented to thought must be "soluble in old experiences" before either can be perceived or comprehended. These phenomena have, of course, been long recognized. Thus, in the *Midsummer Night's Dream* we read:

> "Such tricks hath strong imagination,
> That, if it would but apprehend some joy,
> It comprehends some bringer of the joy;
> Or in the night, imagining some fear,
> How easy is a bush suppos'd a bear!"

Again, in all ambiguous geometrical figures and patterns, a strongly imagined effort to see one form rather than another is usually successful in making us perceive the expected shape.

Prof. James, in his chapter on "Attention," following Lewes states the theory of preperception as follows: "*the only things which we commonly see are those which we preperceive* and the only things which we preperceive are those which have been labelled for us, and the labels stamped into our mind. If we lost our stock of labels we should be intellectually lost in the midst of the world." This doctrine, so vivaciously affirmed, consists of two distinct parts which, if true, are both of fundamental importance to educational theory. The first is that effective observation depends on preliminary knowledge of what we are about to observe; and in some sense this must be true since the trained observer is more efficient in his own department than the untrained. The second statement is that language or an adequate terminology is essential to preperception. It seemed to the writer that both these positions ought to be experimentally examined so as to discover the precise difference made by ideational preparation and whether language is essential to the process of observation[1].

---

[1] J. Ward, *Psychological Principles*, ch. IV.

[2] Third Series. Problem the Second (1879).

II. THE GENERAL PLAN OF INVESTIGATION.

Experiments with these aims in view were performed with over eighty University graduates, mostly honours men, taken in sets ranging from six to twelve in a group. The procedure employed with the last two groups is the most satisfactory and will be described in detail, but as the earlier experiments indicated by their results certain essential improvements in method they will be touched upon here. In general, the experiments were conducted on the following plan. Lantern slides were prepared, showing suits of armour, which were projected on to a screen in front of the subjects who were told to observe the picture as accurately as possible, as they would be asked to describe in writing all they had seen. After the first slide had been closely observed for about one minute it was withdrawn, and the subjects were told to note down immediately every detail they remembered in as concise a form as possible and also to state any general observations they had made. No time limit was imposed and each signed a declaration, before giving up his record, that he had tried but could not recall anything else. The set of subjects was now divided into two groups, one of which had a lecture in which the structure of a suit of armour was explained and illustrated by slides showing drawings of each part and the technical names of the parts were written up on the board. It was found that none of the subjects had any but the vaguest knowledge of armour, derived from hazy recollections of pictures or casual visits to museums, and the technical terms were almost absolutely unknown; only three or four of the total number of students examined knew an odd term or two, but hardly knew exactly to which part it applied. When the lecture was over the two groups were again combined into one set and were shown another slide of armour for one minute; then they recorded their observations as before. Introspective records were also made. In this way it was possible to compare two groups; one with, and the other without definite preperception. Marks were assigned on the following scale. Two were given for each part correctly noted (*e.g.* neck guard, wings on knee); one mark for each position correctly recorded (*e.g.* left foot advanced); and one mark for each correct description (*e.g.* arm guard chased; solerets pointed). The same numbers with a negative sign were assigned for incorrect observations under these heads, *i.e.* marks were subtracted.

---

[1] The term preperception is firmly fixed in psychology in English-speaking countries, but unfortunately covers distinct phenomena; one on the perceptual plane, the other ideational. In Stout's *Manual* the term is equivalent to 'complication,' a purely perceptual process. Ward in his *Principles* deals with the doctrine in his chapter on Ideation though the facts dealt with are mainly perceptual or transitional. James uses the term loosely to include perceptual and ideational experiences indiscriminately. In the present study the word is used in the sense in which it is defined in *Baldwin's Dictionary*, namely as "The ideal representation of an object preceding and facilitating the perception of it."

The first set of subjects examined consisted of twenty-three ex-service graduates of average age 26 years. When the first slide had been shown and records of observations had been made they were divided into groups L and N consisting of eleven and twelve respectively. The lecture was given to group L and immediately afterwards both groups were combined and shown the second slide, and then a third slide; records of observations and introspections being made after each.

For group N the average marks for the three slides were 20, 22 and 25 (the medians being 21, 24, 20); for group L the averages were 30, 38 and 38 (medians 30½, 37½ and 38½). It will be seen that the groups were of unequal merit at the beginning and that the group which had the lecture improved relatively only slightly more, if at all, than the other group. The outstanding feature of these marks is the surprisingly small difference made in preperception by the lecture. The introspective records make clear the reason for this; for, after the second slide, half the subjects in group L complained that the endeavour to remember the technical terms confused them and made observation more difficult. Some of the introspections may be quoted as they illuminate different points. Thus one subject said: "The technical terms were of use only in so far as they broke the suit into pieces thus indicating what parts to look for." Another said: "The knowledge of technical terms enabled me to work systematically," and this was confirmed by several. "The lecture aided the observation in that I used the sequence of the lecture and was able to *anticipate objects, e.g.* I looked at once for the lance rest." This may be compared with the following: "I would not have seen the lance rest if I had not expected it. I had to search for it. The lecture hindered observation because of the technical terms. If more time had been given after the lecture this might not have happened. The lecture, however, was of direct assistance in that after it much less attention was paid to such details as decoration which was immediately dismissed as Milanaise, chasing, etc. In *this* way technical terms helped." The next two records indicate clearly why the lecture made so little difference: "The lecture aided me because (*a*) it prepared me to deal with the suit part by part instead of wondering where to begin; (*b*) it was a guide to parts to look for. The terms helped observation when they were thoroughly known but hindered when I had a difficulty in remembering them." "The half-learnt terms obtruded when observation was going on, tending to a mixture of two endeavours—to observe, and to fix on the right terms; with the result that neither was properly accomplished. In the few cases where the name was recalled without effort, the observation was helped."

It was evident that the chief effect of preperception was to introduce order into the observation and that technical terminology was only of assistance provided that it was thoroughly well known.

III. REFINEMENTS IN METHOD—THE TIME FACTOR

With the next set of sixteen subjects special care was taken to see that the technical terms were well known, for which purpose the lecture was repeated twice and the terms emphasized. Questions were also invited and answered. In order to secure a better comparison the subjects were divided into two groups of equal ability as measured by the marks obtained on the first slide. Two slides only were shown this time. The results, nevertheless, were puzzling since the group that had the lecture seemed to have profited by it astonishingly little compared with the other group, as the following figures indicate. The average marks of group N (the unlectured) for the first and second slide were 17½ and 31 (medians 15 and 29); for group L the averages were 17 and 34 (medians 19 and 33½). A further set of eighteen subjects gave the following averages for initially equivalent groups; group N, 19½ and 37 (medians 20 and 35); group L, 19½ and 51 (medians 21 and 53). This last result is much more what might have been expected *à priori*. There were still baffling results obtained with other subjects owing to the eagerness of the groups who had the lecture to *concentrate on detail and to neglect general features*, which neglect caused a certain group of L subjects, who had the lecture, to score less than its equivalent N group in the second slide. Also several of the men required more practice to adjust themselves to the experimental conditions. The experiment was so interesting to nearly all the subjects that their eagerness militated against the calm which is essential to this kind of work; especially when they were shown the first slide. It was evident, also, that merely being familiar with the structure of armour and the terminology was not sufficient to display the full effects of ideational preparation. The subjects ought not only to learn their lesson but to have a sufficient period to digest it. Adequate time for mental assimilation of the knowledge given by the lecture turned out to be the kernel of the problem of preperception. Attention is facilitated by preperception only when this condition is fulfilled. Time is of the very essence of the problem. During the interval there is an actual stamping in of the previous knowledge, a phenomenon to which Dr Ballard has given the name of Reminiscence. He says: "The belief that the change that takes place in the nervous system during conscious learning is to a certain extent continued when the learning has ceased is forced upon us when we consider those phenomena of reminiscence in which the physical basis is marked and manifest." He gives as instances the improvement of skill in swimming, skating, typewriting, etc., which occurs in the intervals when no practice is being taken; and concludes that "an actual modification of brain structure, of the same nature as that which is supposed to occur during learning, gradually goes on during the interval[1]." The only objection to this view is that the changes are supposed to be purely nervous. There seems no sufficient reason to doubt that mental changes of a similar kind also take place during periods of inactivity. The results of the present series of experiments have convinced the writer that mental changes, leading to better systematization of facts and more adequate assimilation of terms, takes place after the conscious learning has ceased.

IV. THE FINAL METHOD OF INVESTIGATION—SUBJECTIVE ACTIVITY.

The final method of experiment adopted, which proved satisfactory in bringing to light the effects of preperception, may now be described. Sheets of paper were distributed to the subjects, divided into two columns with a subdivision for introspective remarks. The purpose of the experiment was announced as being the attempt to discover the difference made in observation by a preliminary knowledge of what one is about to observe; and the subjects were also told that they would be asked to describe on their sheets everything they could remember. They were likewise informed that suits of armour would be projected from the lantern on to the screen. Slide (1) was then shown on a screen in a darkened room for 1½ minutes. This time was discovered by several preliminary trials to be the optimum. All agreed that the time was sufficient for a complete observation, but when they were asked at the end of the whole experiment for how long they thought the slides had been shown their answers varied from 1 minute to 5 minutes. If a longer time is given several subjects begin to get restless and are apt to let their attention wander; with less time they feel hurried. Before each slide is exhibited it is as well to assure the observer that he will have adequate time and that he need not worry on that account.

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[1] Ballard, "Obliviscence and Reminiscence," pp. 70 ff. and 82. Monograph supplement of this *Journal* (1913).

The slide was then withdrawn and immediately afterwards the subjects were asked to state in one column in brief catalogue form every detail they could recall, and subsequently in the other column any general observations which could not conveniently be considered as detail. Any remarks they wished to make, not of an observational nature, were to be placed in the introspection column. As much time as was wanted was given to making these records, at the end of which the subjects wrote and signed the declaration "I have tried my best but cannot recall anything else." No sheet was accepted without this statement.

What the subjects were really trying to do was to read off from their memory image the details which they had noticed during their observation of the picture. It may be objected that a good visualizer will be able to decipher details which he has not observed, but, as it were, photographed and subsequently developed. But this view has been shown to be a mistake. The best visualizers, who claim to have perfect imagery, are not capable of reading off details which they have not *definitely* attended to. It has been demonstrated, for instance, that such a person claiming to retain a complete mental picture of the front of a building (say) is unable to read off the number of pillars unless he has definitely counted them. Prof. Woodworth, as the result of the study of his own imagery asserts that "it always consisted of facts previously noted." He goes on to say that "an actual situation presents an almost unlimited variety of facts or features, of which an observer notes a few, the rest remaining undiscriminated in the background....Later he may 'remember' the situation, but this is not to reinstate it in its original multiplicity or continuity. He recalls the features which he has observed, or some of them, but not the great mass of them which remained in the background. Lacking this setting or background, he is not in a position to make any fresh observation in recall[1]." Prof. Woodworth's conclusions have been repeatedly confirmed in the course of the present investigation and also in some experiments undertaken by a totally different method by the present writer. *In brief, we cannot study mental*

---

[1] "A Revision of Imageless Thought," *Psychological Review*, XXII (1915).

imagery without reference to mental activity and all that preperception can do is merely to facilitate such activity[1].

The next step in the method was to interchange the records amongst the subjects. The picture was again thrown on to the screen (this time in the lighted room) and they marked each other's papers in accordance with the scheme given above. Every doubtful point was referred to the writer for his final decision and he subsequently examined the sheets. This method of marking has the great advantage that it forces the subjects to examine the picture in detail and stimulates interest and competition amongst the groups, which is necessary in this type of experiment. The method of evaluating the marks must take into account the fact that both groups have some practice in preperception; and this has been done in the calculations made below.

The whole process was repeated with a second slide (2) which was also shown for 1½ minutes and marked in the same way. The marks for the two performances were added together and, on the basis of the totals, all the subjects were divided into two groups of equal ability. For the particular set of fourteen subjects whose results will be considered in detail the combined marks for the two slides were 354 for group L (the lectured group) and 343 for group N (the non-lectured group). It was not possible to get closer totals, as one subject proved so considerably superior to the others. The advantage of combining two sets of readings lies in the fact previously noted that some habituation is required in order to allow several of the subjects to adjust themselves to the experimental conditions.

The lecture, printed in the appendix, was now given to group L, the other group being dismissed. It was repeated twice and the various portions of a suit of armour were shown on the screen, each technical term being written on the board. At the end of the lecture, slide (1) was again projected on to the screen and the parts indicated by name. Finally, the terms were copied into note-books by the subjects who were told to go over them during the following week until they were perfectly familiar with them. Questions were freely asked and answered during the lecture. In this manner the seven subjects of group L were enabled completely to assimilate all the terms and the details of the structure of armour, thus being placed in a most favourable attitude for preperception. All the subjects successfully learnt and could use all the terms freely. Exactly one week later both groups were recombined and two further slides (3 and 4) were exhibited, as before, the details of procedure being the same. When each observation had been recorded and signed they were told to state on the back of their sheets "what they thought of the slide" itself, *i.e.* without comparing it with the others. In this way it was hoped to get some light on the question of aesthetic appreciation in the two groups.

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[1] It is often assumed that the content of a remembered fact must, of necessity, be a sensorial image, visual, auditory, verbal, etc. But there is no convincing evidence whatever for this view, and many professed psychologists suffer from introspective illusions, confusing feelings with images.

At the conclusion of the whole experiment, when the records had been marked and given up, the following questions were answered. Those who had the instruction stated whether and how their observation had been aided or hindered by the lecture and the technical terms; those who did not have the lecture stated whether the absence of a terminology hindered observation and in what manner. They were expressly enjoined to give no theory but simply to describe their experience.

V. THE QUANTITATIVE RESULTS.

The combined marks for each subject for slides (1) and (2) at the first sitting, and for slides (3) and (4) at the second sitting are given below:

Without preliminary knowledge (*Slides* 1 *and* 2).

Group L		Group N	
Subject	Marks	Subject	Marks
A	94	*P*	61
B	54	*Q*	60
C	53	*R*	53
D	51	*S*	57
E	42	*T*	42
F	39	*U*	41
G	21	*V*	29
Mean	50·6	Mean	49
m.d.	14·2	m.d.	10

Coefficient of variation*, 28 Coefficient of variation, 20·4

* *i.e.* $\frac{\text{m.d.}}{\text{Mean}} \times 100$.

A week after the lecture (*Slides* 3 *and* 4).

Group L		Group N	
Subject	Marks	Subject	Marks
A	153	*P*	85
B	114	*Q*	94
C	107	*R*	75
D	107	*S*	55
E	102	*T*	45
F	118	*U*	81
G	82	*V*	67
Mean	111·9	Mean	71·7
m.d.	14·1	m.d.	13·8

Coefficient of variation, 12·6 Coefficient of variation, 19·

The effect of organized preperception can clearly be discerned in the differing variabilities of the groups. For the unlectured group N remains practically constant in this respect; whereas the group L displays a marked decrease in variability. It is important, for the theory of education, to realise that systematic training tends to produce greater uniformity within a group. Group N had, of course, some training in the observation of the first two slides and in marking them, but such training was undirected by precise knowledge, and unsystematic as compared with that of group L. Experiments of this nature may serve to differentiate the relative values of class instruction and individual work especially for weaker pupils; since the decreased variability, as the figures show, is brought about by levelling up the less able whilst the trained group as a whole and individually show better results.

If we desire to compare the trained group with the untrained from the point of view of the relative difference made by preperception, we must resort to the statistical device of using as our unit the standard deviation of the groups. The standard deviations of groups L and N at the outset were 20·6 and 11·1 respectively, yielding an average of 15·8. A week later the deviations were 19·9 and 16 with an average of 17·9. The crude figures in the above tables were converted into multiples of these units, yielding the following numbers when cleared of decimal points.

	Group L	
	Marks (in terms of σ)	
	Slides 1 and 2	Slides 3 and 4
Subject		
A	60	85
B	34	64
C	34	60
D	32	60
E	27	57
F	25	66
G	13	46
Mean	32	63

	Group N	
	Marks (in terms of σ)	
	Slides 1 and 2	Slides 3 and 4
Subject		
P	39	47
Q	38	53
R	34	42
S	36	31
T	27	25
U	26	45
V	18	37
Mean	31	40

As the figures are now comparable, we shall not be far out in assuming that if group L had no lecture they, too, would have risen to an average of 40 instead of 63. Consequently 23 marks is the gain due to preperception; and to find the percentage gain we must start not from their original average mark namely 32, but from 40. This is a gain of 57·5 per cent. To put it otherwise, these figures show that as a result of preperception the trained group is able to observe somewhat over one and a half times as much as the untrained group; which implies, as pointed out previously, *that they are capable of so many more separate definite acts of attention*[1].

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[1] See the discussion on p. 7.

VI. THE QUALITATIVE CONCLUSIONS.

Having thus obtained a numerical estimate of the effects of preliminary knowledge in facilitating attention, it is time to consider the non-quantitative results which are, perhaps, of more significance. In dealing with these use will be made of the records of the whole of the eighty subjects and of their introspections.

One of the most important differences made by preperception is very difficult to describe, as it is so essentially subjective. The observer feels that his mental energy is being more effectively spent and this tends to make him feel more active. Such effective use of mental effort is accompanied by a distinct feeling of satisfaction, which seems to make the effort easier. Thus the person at one and the same time feels that he is more active with less effort. One of the N subjects put the matter in this way: "Lack of terms distinctly hindered my observation. I knew one technical term from reading Scott (tasset) and I always looked first to that part of the armour and had a *feeling* of *sureness* about this. Lack of terms hindered the mental separation of the armour into details; I could only remember each detail as soon as it was separated."

After the lecture the subjects of the L groups took the initiative and went out on a voyage of active exploration to discover the definite parts named; whereas previously the picture, as it were, had the initiative and the person was passively trying to remember what it offered to him. Or, to vary the figure, just as an animal expecting the appearance of its prey is ready to spring with all its muscles pre-adapted for that purpose, so the person expecting to see certain details in a picture has his mind attuned ready to pounce upon them. Owing to this difference of attitude it frequently happened that the elaborate decoration on the last two slides confused or baffled those who had no lecture, so that they lost the wood for the trees; but this never happened with the lectured group who were able to dismiss the decoration with a name, and subsequently to remember its nature. These experiments, therefore, yield confirmation of the view put forward by Dr C. S. Myers that recollection of an observed visual fact may be based on purely verbal imagery without any sensorial image of a visual nature being present[1].

It will be remembered that the subjects were asked, at the end of the experiment, to say how the lecture and the knowledge of terms had helped their perception of the picture. The records show that the greatest help is given in supplying a plan for the observation, *i.e.* in making it more systematic and definite. Recognition becomes more rapid as a result of preperception, and more certain—no subject ever complained that he could not *see* a detail after the lecture was given. The lecture makes the details stand out from the picture; as one subject said, "The ordinary parts were emphasized because one knew where to look for them. Without the lecture it was the curious or outstanding parts that were noticed." The latter part of this statement is open to challenge. In the fourth slide the size of the 'lance rest,' when projected on to the screen, was about two inches in length and, as may be seen from the picture, forms a conspicuous feature; but it is hardly ever seen by a subject who does not know what it means. In fact, it was this object which induced the writer, after the first set experiments, to insist on the declaration that everything that could be remembered had been recorded, as it was difficult to persuade a person otherwise that he had missed so conspicuous a feature. In slide (3) where the lance rest is inconspicuous owing to the ornamentation, it was never overlooked by those who knew what to look for. Finally, the absence of expected parts is immediately noticed; several subjects declaring that they needed only to look for missing parts after the lecture in order to take in the picture. One subject of an N group who had considerable artistic skill pointed out a curious difficulty. He said that he failed to observe the picture accurately, in detail, because he could not separate constructional from decorative lines owing to lack of preliminary knowledge of the structure of armour.

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[1] *Text-book of Experimental Psychology*, p. 141 (1911).

With regard to terminology it became evident that its chief use was to facilitate description, *i.e.* to make it more concise, more 'telescoped.' But this is not all; for frequently the mental image needed a word to fix it, that is to say, the terms formed 'pegs,' as one subject said, on which to 'hang' the detailed observations as they were made. Closely connected with this is the aid given in discriminating parts by the use of words. Words make the dissection of the suit into parts much easier. For this purpose it is essential that the names should be perfectly familiar. The following introspective record illustrates this: "Terms did not help me because I thought of the armour in terms of my own making. My method was to consider the parts of the body covered and *then* the name. Breast plate was 'breast plate' in my mind and 'cuirass' only by an effort of memory. This does not apply to 'greaves.' since I happen to know this term as a literary one."

We can best, perhaps, see the utility of terms by noticing what the subjects who had no lecture reported. "With terms," said one, "I would have tabulated all the possible parts into a generalized suit of armour and I should have then gone through the given suit checking these off." In other words lack of terms prevents generalization. Another subject stated that "Lack of knowledge of structure and terms hindered me owing to lack of definite starting points, and definite systematization in classifying parts. Also by necessitating the use of roundabout phrases, which was apt to turn my concentration on the visual image to concentration on the terms expressing it." Several others pointed out the necessity of putting their observations into language, whilst the slide was shown, in order to recall the image later. Thus one particularly conscientious observer made the remark: "I could have mentioned several other points but having no terms I was quite unable to express what I had noticed." He really meant that his memory image failed because he had no terms with which to retain it. Another remarked that he had a difficulty in retaining the relation of one part to another and he thought that terms would have helped him in this respect. Some few, however, denied all this and maintained that the absence of terms simply made their descriptions more clumsy. We are here probably dealing with ultimate differences in mental-make-up.

VII. PREPERCEPTION AND AESTHETICS.

A final tentative suggestion is offered as to the aesthetic significance of these experiments; as it is hoped that the method will ultimately yield facts bearing on aesthetic education. One difficulty militates against any very precise results. Any mention of appreciation during the experiment would, inevitably, interfere with free observation and introduce disturbing factors. For this reason it was decided to approach the matter indirectly, by observing the number and nature of the aesthetic remarks made spontaneously by the subjects.

The fourteen subjects of the two final groups were asked to state with reference to the second, third and fourth picture, what they "thought of the slide." They were told in each case not to compare the slides but to treat the one under observation by itself, and to state whatever occurred to them with regard to it. Readers of this *Journal* are acquainted with Mr E. Bullough's four perceptive attitudes, which may be referred to by the following letters: *A*, The objective aspect; *B*, the intrasubjective; *C*, the associative; and *D*, the character aspect[1].

The objective attitude is that taken by subjects who refer to such aspects as the poorness or stiffness of the picture or whether it is a line drawing or photograph, and so on. The intrasubjective attitude is shown by such comments as exciting, stimulating or cold, etc., these physiological effects being produced by the slide on the subject. Those who adopt the associative attitude refer to tournaments, chivalry, modern weapons and so forth. Finally the character aspect is revealed by such remarks as fierce, stubborn, valiant, etc., which indicate some human characteristic possessed by the picture.

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[1] Bullough, "The Perceptive Problem, etc.," vols. II and III. Myers and Valentine, "Individual Differences in Attitudes towards tones," vol. VII. Myers, "Individual Differences in Listening to Music," vol. XIII.

There seems to be some considerable doubt as to the aesthetic value of these aspects, more especially as Mr Bullough apparently regards the objective attitude as practically extra-aesthetic. Again the value of the associative aspect depends on whether the associations aroused are completely fused with the picture or not. For our present purpose it is sufficient to regard *A*, *B*, *C* and *D* as forming a scale of aesthetic values from the lowest to the highest.

With regard to the second slide the subjects of group L were classified roughly as 5*A* and 2*C*; and group N as 4*A* and 2*C*. One subject gave no reply; so that both groups were of about equal merit as regards aesthetic appreciation. For the third slide the numbers for group L were 5*A* and 1*D* and for group *N* there were 3*A*, 2*B* and 4*C*. These figures show that the subjects gave distinct indications of belonging to mixed types, so that some had to be counted in two or more categories. The relatively larger number of the objective type in group L, together with the paucity of other types, points to the fact that the lecture by forcing attention to the details of observation fails to allow of the development of aesthetic appreciation. For slide (4) the figures for group L were 3*A*, 2*B*, 1*C*, 5*D* and for group N they were 2*A*, 1*B*, 2*C*, 6*D*[1]. The numbers of mixed types has increased in both groups; greater knowledge of detail whether systematically or incidentally acquired has now stimulated aesthetic appreciation. Similar results were obtained from earlier sets of subjects. The final conclusion may perhaps be stated tentatively thus; too great concentration on analysing detail may interfere with the aesthetic attitude at the outset, but as the result of increasing familiarity this effect seems to disappear.

APPENDIX.

LECTURE ON ARMOUR.

A gradual progress is evident in the improvement of arms from the earliest times. Starting from the tenth century we are able to follow step by step the gradual change in defensive armour. The mail shirt remained in use for over five hundred years, and was replaced by complete plate armour only after a transition period in which coats of mail

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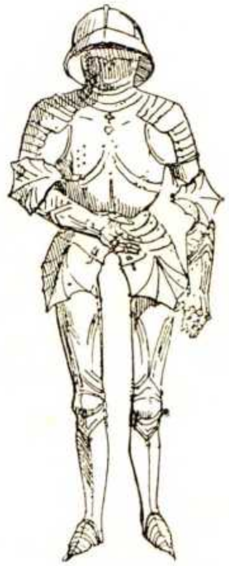
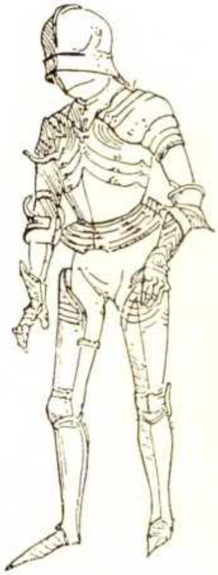
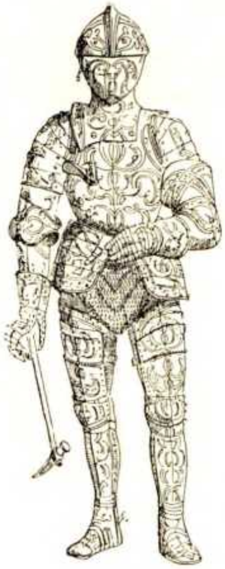
[1] The large number of *D* remarks is, doubtless, due to the fact that it is difficult to avoid giving a 'character' aspect to a plan of a human figure; which fact may be held somewhat to vitiate the results.



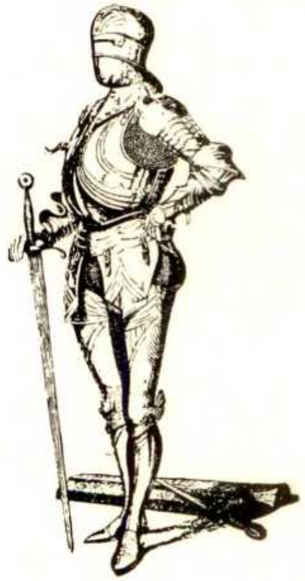
Slide 1.



Slide 2.



Slide 3.



Slide 4

FOX.—A Study in Preperception.

partly composed of plates of iron were used. The armour of the sixteenth century had beautiful flutings called *Milanaise*; and in the second half of this century was adorned with chased engravings.

Our slides are concerned with the perfected plate armour showing some traces of the old coats of mail as vestiges of the earlier covering.

The casque or helmet was of diverse shapes and of great antiquity but the real bowl-shaped helm, the *salade*, only came into use in the fifteenth century. This sometimes had a *neck guard* at the back and had a moveable or fixed *vizor* or sight piece, which however was usually so short (end of nose) that a *beaver*, or chin piece, was necessary to guard the chin, neck and mouth. Later came a rounded helmet or *armet*, the crown of which was sometimes crested and sometimes the chin piece and gorget were fastened to it.

The neck collar, made of leather, which supported the rest of the body armour and lying underneath (therefore never seen) supported the

Gorget, or throat guard, sometimes with *pauldrons*, or shoulder plates, attached. Often the pauldron was a separate part.

Below was the *cuirass*, or breast plate, which protected the chest and was often made with a *prominent ridge*. This had usually a *lance rest*, which was placed on the right of the breast plate and was used to support the lance, when on horseback.

Below the breast-plate was the waist piece or great *brayette*, that part of the armour which guarded the abdomen. It was composed of steel plates and usually ended in

Tassets, designed to protect the upper part of the thighs, and strapped with thongs to the brayette.

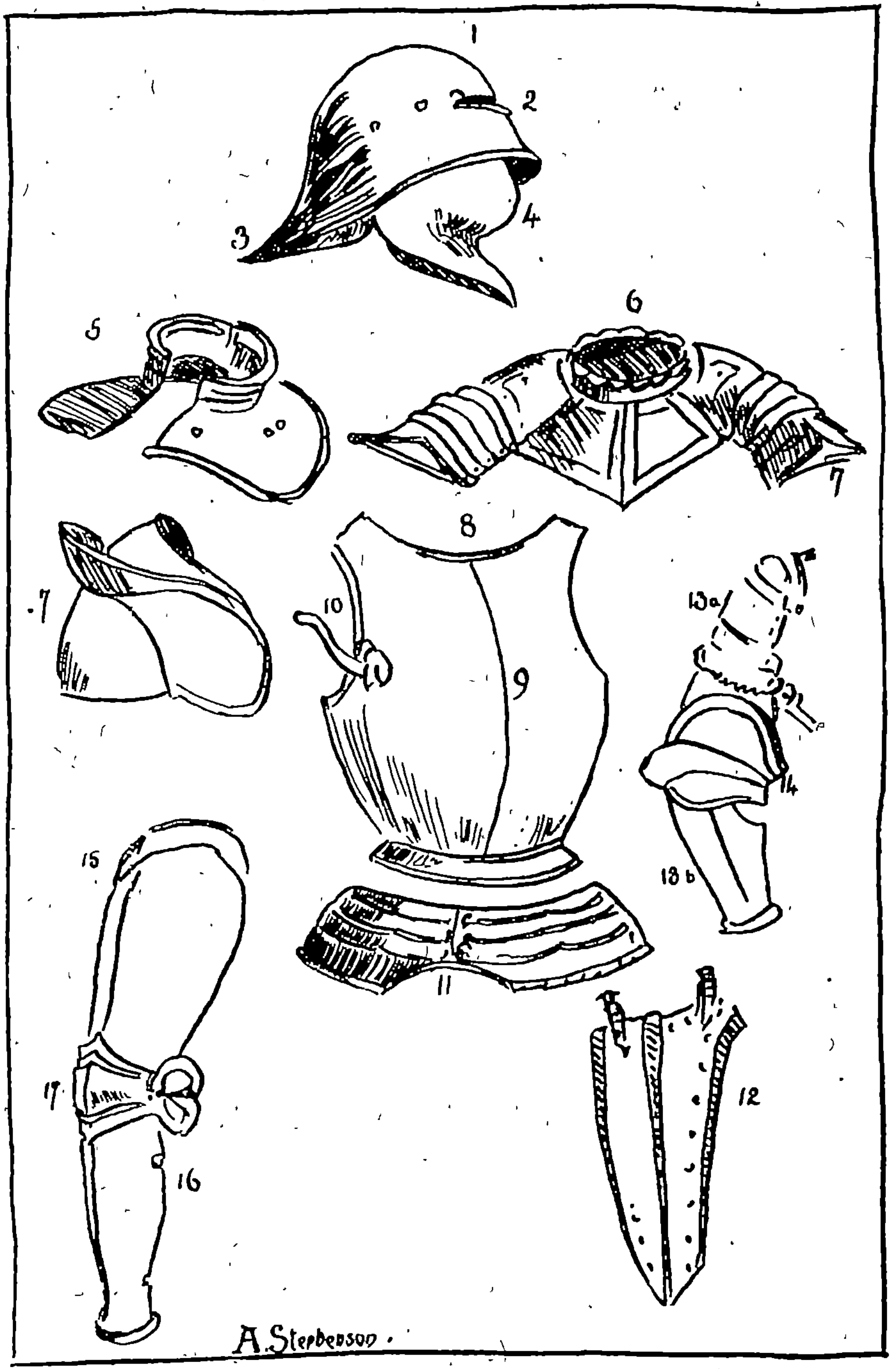
The arms were protected by the *arm guards*, composed of upper and lower armplates joined together by the *elbow piece*, which sometimes had prominent wings.

The legs were guarded by the *cuisses* on thigh, and the *greaves* on the shins, joined by the *knee plate*, sometimes with wings.

In addition there were *gauntlets*, with or without separately articulated fingers; and the armed shoes or *solerets*, sometimes with long points.

KEY TO FIGURE.

1 Salade.
2 Vizor.
3 Neck Guard.
4 Beaver.
5 Neck Collar.
6 Gorget.
7 Pauldrons.
8 Cuirass.
9 Ridge.
10 Lance rest.
11 Great Brayette.
12 Tasset.
13 a Upper Arm Guard.
13 b Lower Arm Guard.
14 Elbow piece.
15 Cuisse.
16 Greaves.
17 Knee plate.



(*Manuscript received* 20 *February* 1924.)

THE SAMPLING ERROR IN THE THEORY OF TWO FACTORS

By C. SPEARMAN and K. HOLZINGER.

THERE has been much discussion—both in this *Journal* and elsewhere—as to the validity of the theory of "Two Factors." According to this theory each ability concerned is capable of resolution into two factors, the one being specific to this ability alone, whereas the other is common to all of them. A large amount of experimental data has been examined, as to how far it conforms with this theory.

So far, however, the sole available criterion of conformity has not been the correct one. It has only been a substitute that could supply good approximations under certain circumstances, but was liable to mislead under others. The present paper proposes to make available for use the correct criterion. In this way a problem of outstanding importance would appear to be brought markedly nearer to solution.

The said criterion consists in the relation

$$\frac{r_{13}}{r_{14}} = \frac{r_{23}}{r_{24}} \qquad \ldots\ldots(1),$$

where the suffixes indicate any of the abilities at issue. This relation has been proved to hold when, and only when, the abilities are resolvable into the said two factors[1].

Now, the great difficulty of applying this criterion derives from the correlations being disturbed by the errors of sampling (those whose general magnitude is measured by the "probable errors"). When (1) is exact for the true correlations, how much may it be expected to deviate for the correlations as affected by the errors of sampling? On ascertaining this deviation which should be expected theoretically, we can at once compare it with that which is actually observed. According as the two amounts coincide or not, the theory of Two Factors shows itself to be valid or invalid.

Let us, then, assume (1) for the true correlations, or those which derive from some entire "population," so that here

$$F \equiv r_{13}\, r_{24} - r_{23}\, r_{14} = 0 \qquad \ldots\ldots(2).$$

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[1] See Spearman, *Proc. Roy. Soc.* A, vol. CI, 1922, pp. 97–100.

If the population is replaced by a sample, and if d symbolizes sampling errors, each r in (2) has to be replaced by $r + dr$, so that we get

$$dF = \{r_{24}dr_{13} + r_{13}dr_{24} - r_{14}dr_{23} - r_{23}dr_{14}\} + \{dr_{13}dr_{24} - dr_{23}dr_{14}\} \quad \ldots(3)$$

$$\equiv \{A\} + \{B\}, \text{ and}$$

$$(dF)^2 = A^2 + 2AB + B^2 \quad \ldots\ldots(4).$$

To obtain σ^2_F, we must sum for all different samples and divide by their number. We will do this in turn for each of the three expressions on the right side of (4).

Thus,

$$\left.\begin{aligned}\Sigma(A^2)/\nu = {} & r^2_{24}\sigma^2_{r_{13}} + r^2_{13}\sigma^2_{r_{24}} + r^2_{14}\sigma^2_{r_{23}} + r^2_{23}\sigma^2_{r_{14}} \\ & + 2r_{24}r_{13}\sigma_{r_{24}}\sigma_{r_{13}}R_{r_{24}r_{13}} + 2r_{14}r_{23}\sigma_{r_{14}}\sigma_{r_{23}}R_{r_{14}r_{23}} - 2r_{13}r_{23}\sigma_{r_{14}}\sigma_{r_{24}}R_{r_{14}r_{24}} \\ & - 2r_{14}r_{24}\sigma_{r_{13}}\sigma_{r_{23}}R_{r_{13}r_{23}} - 2r_{23}r_{24}\sigma_{r_{13}}\sigma_{r_{14}}R_{r_{13}r_{14}} - 2r_{13}r_{14}\sigma_{r_{23}}\sigma_{r_{24}}R_{r_{23}r_{24}}\end{aligned}\right\} (5),$$

where ν is the number of samples. The R's and the σ's can for normal distribution be obtained from the formulas of Filon and Pearson[1].

Turning to $\Sigma(AB)/\nu$, this gives terms of the form $r\Sigma(dy.dz.dw)/\nu$, each of which, with normal distribution, is zero[2], so that

$$\Sigma(AB)/\nu = 0.$$

Finally,

$$\begin{aligned}\Sigma(B^2)/\nu = {} & \{\Sigma[(dr_{13})^2(dr_{24})^2] + \Sigma[(dr_{23})^2(dr_{14})^2] \\ & - 2\Sigma[dr_{13}dr_{24}dr_{23}dr_{14}]\}/\nu \quad \ldots\ldots(6)\end{aligned}$$

$$\left.\begin{aligned} = {} & \sigma^2_{r_{13}}\sigma^2_{r_{24}}(1 + 2R_{r_{13}r_{14}}) + \sigma^2_{r_{23}}\sigma^2_{r_{14}}(1 + 2R_{r_{23}r_{14}}) \\ & - 2\sigma_{r_{13}}\sigma_{r_{24}}\sigma_{r_{23}}\sigma_{r_{14}}[R_{r_{13}r_{14}}R_{r_{23}r_{14}} + R_{r_{24}r_{23}}R_{r_{13}r_{14}} + R_{r_{13}r_{23}}R_{r_{24}r_{14}}]\end{aligned}\right\} (7)^3.$$

Since the value on the right of (7) contains only fourth-order terms of the σ's, it can be neglected in comparison with $\Sigma(A^2)/\nu$, unless either N (the number of cases in the sample) or the r's are very small. But in the latter case the terms involving R become negligible. Hence, unless N is very small—say, under 50—we get, with good approximation,

$$\Sigma(B^2)/\nu = \sigma^2_{r_{13}}\sigma^2_{r_{24}} + \sigma^2_{r_{23}}\sigma^2_{r_{14}} \quad \ldots\ldots(8).$$

We are now able to put together (4), (5), (6) and (8). Utilizing (2), there ensues by straightforward algebra

$$\left.\begin{aligned}\sigma^2_F = {} & \frac{1}{N}[r^2_{13} + r^2_{14} + r^2_{23} + r^2_{24} - 2(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} \\ & + r_{23}r_{24}r_{34}) + 4r_{12}r_{13}r_{24}r_{34}] \\ & + \frac{1}{N^2}[(1 - r^2_{13})^2(1 - r^2_{24})^2 + (1 - r^2_{23})^2(1 - r^2_{14})^2]\end{aligned}\right\} (9).$$

And of course the probable error of F is got by taking the root and multiplying by ·6745.

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[1] *Phil. Trans. Roy. Soc. Lond.* A, CXCI, 262.

[2] See Isserlis, *Biometrika*, vol. XII.

[3] *Ibid.*

An approximation that may often suffice can be got from replacing each r by their mean. In this way,

$$\sigma^2_F = 4r^2(1-r)^2/N + 2(1-r^2)^4/N^2 \qquad \text{......(10).}$$

In (9) and (10) the term divided by N^2 can be neglected unless both N and the r's are small. Then (10) gives simply

$$\sigma_F = 2r(1-r)/\sqrt{N}\,^1 \qquad \text{......(11).}$$

As a border-line case where the approximation is of dubious adequacy, let $N = 70$ and $r = \cdot 30$. Thereupon the P.E. by (10) is ·036, but by (11) ·034.

It may be remarked that in (9) the upper line on the right can be expressed more simply in terms of the correlations between the four abilities concerned and the General Factor. For this line reduces to

$$\frac{1}{N}(r^2_{1g} + r^2_{2g} - 2r_{1g}r_{2g})(r^2_{3g} + r^2_{4g} - 2r_{3g}r_{4g}),$$

where g denotes the general factor.

Probable Errors of $F = r_{13}r_{24} - r_{23}r_{14}$, *where* r *in the formula is the average value in* F.

$$\text{P.E.}_F = 1{\cdot}349\sqrt{\frac{r^2(1-r)^2}{N} + \frac{(1-r^2)^4}{2N^2}}.$$

r	N			r	N		
	50	70	100		50	70	100
·00	·0183	·0136	·0095	·50	·0489	·0410	·0341
·02	·0194	·0140	·0099	·52	·0487	·0408	·0341
·04	·0204	·0149	·0108	·54	·0484	·0407	·0338
·06	·0218	·0163	·0121	·56	·0479	·0402	·0336
·08	·0235	·0180	·0137	·58	·0473	·0398	·0332
·10	·0254	·0197	·0153	·60	·0465	·0391	·0326
·12	·0273	·0215	·0170	·62	·0455	·0383	·0320
·14	·0294	·0234	·0186	·64	·0445	·0374	·0313
·16	·0314	·0253	·0202	·66	·0432	·0365	·0305
·18	·0334	·0270	·0218	·68	·0419	·0353	·0295
·20	·0352	·0287	·0233	·70	·0404	·0341	·0284
·22	·0370	·0303	·0247	·72	·0388	·0327	·0273
·24	·0387	·0318	·0260	·74	·0369	·0311	·0261
·26	·0403	·0332	·0273	·76	·0350	·0295	·0247
·28	·0418	·0345	·0284	·78	·0328	·0278	·0231
·30	·0431	·0357	·0294	·80	·0306	·0259	·0216
·32	·0443	·0368	·0304	·82	·0283	·0238	·0199
·34	·0453	·0377	·0312	·84	·0257	·0217	·0181
·36	·0463	·0385	·0319	·86	·0230	·0194	·0162
·38	·0470	·0393	·0326	·88	·0201	·0170	·0142
·40	·0477	·0399	·0331	·90	·0172	·0145	·0121
·42	·0483	·0404	·0335	·92	·0140	·0119	·0099
·44	·0486	·0407	·0338	·94	·0108	·0091	·0076
·46	·0489	·0410	·0340	·96	·0073	·0062	·0052
·48	·0489	·0410	·0342	·98	·0037	·0032	·0026

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[1] This value was communicated by the authors at a meeting of the Brit. Psych. Soc., 23rd February, 1924.

(*Manuscript received* 11 *March* 1924.)

A NOTE ON THE CORRELATIONS OF SUMS

BY LL. WYNN JONES.

IN the testing of intelligence or vocational aptitude or in other investigations where a number of tests is employed, it is often important to know how each test correlates with all the tests pooled together. The omission of this step might lead to results analogous to those experienced by a commander who trusted to the reports of a dozen spies, some of whom were unreliable. The procedure to be adopted has been indicated by Professor Spearman in 1913, the following well-known formula being established[1]:

$$r_{(a_1+a_2+\dots+a_p)(b_1+b_2+\dots+b_q)} = \frac{\sqrt{pq}\,.\,\bar{r}_{ab}}{\sqrt{1+(p-1)\,\bar{r}_{aa}}\sqrt{1+(q-1)\,\bar{r}_{bb}}}.$$

The aim of this note is to point out simple corollaries[2] which follow immediately from this formula and which were derived by the writer when working in this field under the direction and guidance of Professor Spearman. Any time-saving device in this work is useful, and the writer has often had occasion to use the corollaries in the course of the last five years. In each case the formula gives the result that would ensue if the standard deviations were made equal before the series are compounded. This is generally the most useful case.

Case 1. Let there be p tests, and, assuming the intercorrelations to have been calculated, suppose that the correlation between any one of them and the pool is required. With the above notation

$$r_{(a_1+a_2+\dots+a_p)\,b} = \frac{\sqrt{p}\,.\,\bar{r}_{ab}}{\sqrt{1+(p-1)\,\bar{r}_{aa}}},$$

where $b = a_p$; $\bar{r}_{ab}$ is the mean correlation between all different pairs of an a and a b; $\bar{r}_{aa}$ is the mean correlation between all different pairs of a's. Further, let

$$\begin{aligned} S_1 &= r_{12} + r_{13} + \dots + r_{1p} \\ S_2 &= r_{21} + r_{23} + \dots + r_{2p} \\ &\dots\dots\dots\dots\dots\dots\dots\dots \\ S_p &= r_{p1} + r_{p2} + \dots + r_{p\,(p-1)} \\ S &= S_1 + S_2 + \dots + S_p \end{aligned}$$

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[1] This *Journal*, 1913, v, 417.

[2] Given in a paper read before Section J (Psychology) at the British Association Meeting, Hull, 1922.

Then $$\bar{r}_{ab} = \frac{r_{1p} + r_{2p} + \dots + r_{pp}}{p} = \frac{S_p + 1}{p}$$

and $$\bar{r}_{aa} = \frac{S}{p\,(p-1)}.$$

Hence $$r_{(a_1+a_2+\dots+a_p)\,a_p} = \frac{S_p + 1}{\sqrt{p + S}} \qquad \text{......(1).}$$

Case 2. Let there be p tests, and, assuming the intercorrelations to have been calculated, suppose the correlation between any one of them and the pool of the rest is required. This test might affect the pool strongly and it might in consequence be desired to ascertain the correlation when this test is not included in the pool. We get

$$r_{(a_1+a_2+\dots+a_{p-1})\,b} = \frac{\sqrt{p-1}\,.\,\bar{r}'_{ab}}{\sqrt{1 + (p-2)\,\bar{r}'_{aa}}},$$

where $b = a_p$; r'_{ab} is the mean correlation between all different pairs of an a and a b; $\bar{r}'_{aa}$ is the mean correlation between all different pairs of a's. Now

$$\bar{r}'_{ab} = \frac{r_{1p} + r_{2p} + \dots + r_{(p-1)\,p}}{p-1} = \frac{S_p}{p-1},$$

$$\bar{r}'_{aa} = \frac{\Sigma r_{aa}}{\frac{(p-1)(p-2)}{2}} = \frac{S - 2S_p}{(p-1)(p-2)}.$$

The required correlation is therefore given by

$$r_{(a_1+a_2+\dots+a_{p-1})\,a_p} = \frac{S_p}{\sqrt{p - 1 + S - 2S_p}} \qquad \text{......(2).}$$

Case 3. Suppose the correlation between any one of the tests with the pool is known and that its correlation with the pool of the rest is required.

Let r_p, r_{p-1} be the correlations of a_p with the pool and with the pool of the rest respectively.

Then, from (1) and (2)

$$r_p = \frac{S_p + 1}{\sqrt{p + S}}; \quad r_{p-1} = \frac{S_p}{\sqrt{p - 1 + S - 2Sp}}$$

and by substitution

$$r_{p-1} = \frac{S_p}{\sqrt{\left(\frac{S_p + 1}{r_p}\right)^2 - 2S_p - 1}} \qquad \text{......(3)}$$

and r_{p-1} may therefore be calculated when the sum of the correlations of the test with each of the others is known.

φ 16297

Illustration. The following table shows the results obtained when the Seashore Tests of Musical Talent were given twice to fifty-four Standard 7 girls in a Leeds school. No claim is made that the intercorrelations obtained here have any general validity for Standard 7 girls, but they serve to illustrate the simplicity of the arithmetic involved. The correlations were first calculated by the Foot-rule method and converted to the values given in the table as demanded by the formula

$$r = 2\cos\frac{\pi}{3}(1 - R) - 1.$$

	Sense of Pitch	Sense of Intensity	Sense of Time	Sense of Consonance	Tonal Memory
Sense of Pitch ...	—	0·33	0·47	0·20	0·49
Sense of Intensity...	0·33	—	0·47	0·33	0·48
Sense of Time ...	0·47	0·47	—	0·26	0·48
Sense of Consonance	0·20	0·33	0·26	—	0·34
Tonal Memory ...	0·49	0·48	0·48	0·34	—

1·49 + 1·61 + 1·68 + 1·13 + 1·79 = 7·7

From (1),

$$r_{\text{pool . pitch}} = \frac{1{\cdot}49 + 1}{\sqrt{5 + 7{\cdot}7}} = {\cdot}70; \quad r_{\text{pool . intensity}} = \frac{1{\cdot}61 + 1}{\sqrt{5 + 7{\cdot}7}} = {\cdot}73;$$

$$r_{\text{pool . time}} = {\cdot}75; \quad r_{\text{pool . consonance}} = {\cdot}60; \quad r_{\text{pool . memory}} = {\cdot}78.$$

From (2),

$$r_{\text{pool of rest . pitch}} = \frac{1{\cdot}49}{\sqrt{4 + 7{\cdot}7 - 2 \times 1{\cdot}49}} = {\cdot}63;$$

$$r_{\text{pool of rest . intensity}} = \frac{1{\cdot}61}{\sqrt{4 + 7{\cdot}7 - 2 \times 1{\cdot}61}} = {\cdot}55; \quad r_{\text{pool of rest . time}} = {\cdot}58;$$

$$r_{\text{pool of rest . consonance}} = {\cdot}37; \quad r_{\text{pool of rest . memory}} = {\cdot}63.$$

Thus the comparative lowness of the correlation of the sense of consonance with the pool is accentuated when the test itself is excluded from the pool.

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THE PSYCHO-GALVANIC PHENOMENON IN DREAM ANALYSIS[1]

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(From the Department of Psychology, University of Manchester.)

I. Aim of Investigation.

The ultimate aim of the present research is to examine, in what seems to be a new way, some assumptions which underlie current views of the dream. One of these concerns the relevance of different ideas which are brought up in free-association.

Everyone who has honestly tried to allow his mind to associate freely, and for a long period, from elements in the manifest content of a few of his own dreams—and on this important matter, nobody else is likely to contribute helpful views—will have found that, of the numerous free-associations which arise, some prove to be more important than others for an understanding of the dream. To put this in another way, while every idea called up by association, from elements in the manifest content, must obviously, in one sense, be relevant, some associations prove to be more intimately connected than others with the latent content of the dream—*i.e.* to be more relevant. Relevance exists in many degrees, and perhaps, in different qualities[2].

This relative relevance may be illustrated by an example. One of our subjects was attempting to analyse a dream, which depicts a cruel action by his uncle. Among the memories brought up during free-association was that of a recent athletic meeting, at which the dreamer had been a spectator. He had seen many famous athletes there, and remembers vividly their appearance. Yet a player who is by no means in the first flight was very obtrusive during the chain of free-associations, and the entry of the idea of this person into consciousness was accompanied by an emotional tone of intense displeasure. The subject then remembered that even a newspaper picture of this man had produced in him a similar emotion a day or two before. He then saw that in the dream the uncle is behaving as this particular person once did in reality, when an account of his conduct appeared in the newspapers. Many other bonds of similarity between the uncle and the athlete then appeared; not only striking external resemblances of appearance but internal connexions of the kind familiar to dream-investigators.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

[2] One of us has suggested that logical and affective relevance are usually distinguishable from a psychological aspect; that the latter is more common, and that often the former may be logical only, so to speak, by accident, being a special case of affective congruity in the mind of a person to whom illogical thinking is unpleasant. (*Remembering and Forgetting,* London, 1922, pp. 138–9.)

It seems reasonable to believe that while all the incidents of the athletic meeting which were revived in free-association were, in a sense, relevant, the memory of this particular player was more relevant than the others and was more significant for the interpretation of the dream.

Now, it is a familiar fact that such significant elements in the chain are often affectively toned, and that this affect makes the subject aware that they are important. Moreover, such elements are frequently factors in dream-condensations. For both these reasons one often expects to find in them keys to the dream-interpretation.

Yet it cannot be assumed that *all* associations which eventually prove to be specially significant for the dream interpretation must be strongly affectively toned when they enter consciousness. Indeed, there may be reasons for believing that there is no close correlation between the two characteristics. But since the psycho-galvanic phenomenon often indicates bodily changes of which the subject is unaware, it might be useful in the investigation of such associations.

The following are some ways in which the psycho-galvanic phenomenon might be of use; to discover

(*a*) whether any items in the dream's manifest content, which have not impressed the subject as important, significant, or particularly relevant, are accompanied by psycho-galvanic disturbances when the record of the dream is read aloud by him;

(*b*) whether ideas, which, in the waking state, arise in association with items of the above kind, prove to be significant for dream analysis, or to produce unusual emotion;

(*c*) whether the free-associations described under (*b*), when they produce psycho-galvanic deflexions, cause deflexions greater or smaller than those arising from elements in the manifest content, the significance or emotional colouring of which was overtly recognised by waking consciousness.

Another interesting possibility was suggested after considering a discussion of a paper read by Dr W. H. R. Rivers to the British Psychological Society[1].

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[1] This paper subsequently formed the substance of Chapter IV in *Conflict and Dream*, London, 1923.

The question under consideration was what may be believed to happen in one's mind and body, when a sudden discovery, made during auto-analysis of a dream, causes one to feel that unusually important light is being thrown upon the dream interpretation. The general opinion seemed to be that at such times an affective experience is felt which is very different in intensity, and probably in quality, from the usual experience of ordinary ideas following each other, in a train of association. If it be admitted that these latter, 'ordinary,' associations, as they may be called, are affectively toned at all, then the association which casts particular light on the dream seems to be accompanied by a greater intensity of affect, and perhaps by an unexpectedness in its flavour, or 'bouquet.'

If one may judge from auto-analysis, sudden realisation of the meaning of a symbol or condensation seems to be accompanied by a state of excitement, coloured by emotion or by a blend of emotions. It seems doubtful whether this total condition is a replica of the affect originally characterising the experience which is now recalled as a memory. Certainly it is not emotion remembered in tranquillity. Rather is it streaked with excitement like that which is stirred up when two trains of association, between which no connexion has hitherto been visible, suddenly display themselves as different aspects of the same theme. This may be because it is akin to, or indeed *is*, that kind of excitement which heralds the sudden collision and interpenetration of two different trains of thought in the mind of the scientist or of the poet and often banishes his sleep.

Not less importantly, this excitement may be the final sparkle in the delicate dry flavour of the intellectual joke. Those who fabricate them, knowing, as one of them writes, that it is more blessed to guess than to be told, leave at least one gap unbridged. Perhaps the pleasurable thrill which is vouchsafed to the hearers who can span this gulf and the unpleasurable lack of it which vexes those who cannot, imply that Nature's abhorrence of a vacuum extends to the ellipsis.

To the method of the psycho-galvanic phenomenon we looked for help in investigating this special kind of excitement and to the dream for material combustible enough to promise it.

II. MODIFICATIONS OF ORIGINAL AIM.

The following procedure was adopted. The subject, acquainted or unacquainted with the experience of dream analysis (fortunately both kinds of subject were available), but being one who dreamt frequently, was asked to write down a dream in the ordinary way, immediately after awaking. On the same day, if possible, he analysed part of the dream, taken from this record, writing down privately his free-associations. Sometimes, however, older dream records were deliberately employed, and in one case a record some years old. These, of course, are modifications of the investigation, with obvious significance of their own.

The apparatus used was a dial bridge with two resistances of 0, 10, 100 or 1000 ohms and one from 1–10,000 ohms, two dry cells, and a mirror galvanometer, with a suitable shunt, throwing a spot of light on to a screen from a 100 c.p. Pointolite lamp. These were connected as shown in Fig 1, in which the subject acts as the fourth resistance in a Wheatstone Bridge. The two smaller resistances were sometimes given the values shown in the diagram because the subject's resistance was found to be between 10,000 and 100,000. In other cases, however, the subject's resistance was less than 10,000 ohms, and these resistances were then made equal. The electrodes (two bronze discs 1¼ inches in diameter) were enclosed in small bags soaked in a salt solution, covered

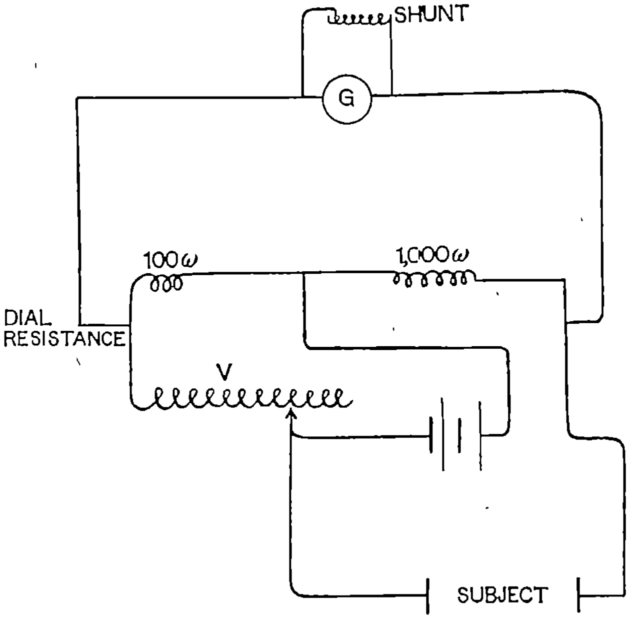

Fig. 1.

with oil-silk to prevent them from drying during the course of the experiment, and pressed firmly on the back and palm of the hand by wads of cotton-wool enclosed in a bandage. The variable resistance was adjusted until no current was flowing through the galvanometer. Changes of the resistance of the subject were then shown by deflexions in the galvanometer. The resistance of the shunt was adjusted until these were of convenient size.

It was found that emotional changes resulted in a sudden decrease of the subject's resistance, which was followed by a comparatively slow increase towards its original value. In addition to this, there was a decrease of the subject's resistance which went on through the whole course of a single experiment. This effect was at first very rapid, but became slow enough not to interfere with the experiment after the electrodes had been attached for about five minutes. We attribute it to the gradual decrease of the skin's resistance through its progressive soaking by the salt solution[1]. The effect was great enough to mask any increase of the subject's resistance through polarisation effects. For this reason, and because such effects had no bearing on the phenomena it was our purpose to observe—the sudden 'kicks' of the galvanometer—we made no attempt to use non-polarisable electrodes. These would have introduced into the experiment an appearance of accuracy in fact unattainable, and, if attainable, irrelevant to the purpose of the experiment.

The analysis and observation of the subject's psycho-galvanic phenomenon were carried out in the following way. He was seated in a Morris chair in a fairly quiet and partially darkened room. His left arm was fixed as comfortably as possible in the arm-holder of a Kraepelin ergograph. The left hand was then connected, as described above, with the psycho-galvanic apparatus, so that variations in the body's electrical resistance would be shown by the movements of the spot of light on the screen; unseen by the subject. Two experimenters were employed, one managing the apparatus and tapping out the time intervals, and the other recording the deviations of the spot, and noting whether they were gradual or sudden, swings or 'kicks.'

The subject, his electrical resistance having been balanced, was given one or two sound stimuli of fairly considerable intensity (dropping the lid of a desk) in order to make sure that the apparatus was working satisfactorily. The latent time for such a stimulus was generally very constant for the same subject, and indeed for all subjects, being between 2 and 3 seconds. He was then asked to copy, on a pad of paper placed comfortably before him, the dream record. (The way in which the time intervals were marked will be discussed later.) Deflexions occurring during this time were noted. After this he was asked to read aloud what he had written.

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[1] One of us, R. H. T., has since performed experiments which suggest that this is not the correct explanation and show that the resistance of some subjects rises instead of falls.

The experimenters then examined the deflexions corresponding to the periods of writing and speaking. Any parts of the manifest content which were accompanied by unusually great deflexions were chosen as the starting place for free-association[1].

The nature of free-association was then explained to the subject in simple terms, whether he was acquainted with dream analysis or not. Examples of both directed- and free-association were given. He was exhorted to let his mind run as freely as possible, and uncensored in any way; was assured that he could withhold or destroy any, or all, of the notes made in the room, and that they would not be seen by either of the experimenters without his consent. He was then asked to associate freely from a point in the manifest content chosen by the experimenter, writing down as quickly and easily as he could, all that came into consciousness.

The initial and final resistances of the subject were noted, and in order to provide a basis for comparison of the amount of the deflexions of different subjects, observations were made (by sudden changes in both directions of the main variable resistance) of the amount of spot-deflexion corresponding to a change of 10,000 ohms in the resistance of the subject.

It should be noted that, in the diagrams accompanying this paper, the unit of time used was 10 secs. This was represented on the original diagrams (which have been reduced in reproduction) by a distance of $\frac{1}{10}$ inch. A 'kick' of the galvanometer normally took place in a fraction of this time, so that in the diagrams, compression has taken place which obscures the contrast between the rapidity of the kick and the comparative slowness of the subsequent increase of resistance. An improved method of making the diagrams will remove this source of obscurity from our future work.

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[1] It should be pointed out that in the present stage of the investigation only a little value can be attached to this procedure, since we do not know (*a*) the time-lag of the deflexion behind the appearance of the thought in consciousness; (*b*) whether the deflexion may not perhaps precede, or be simultaneous with, the appearance in consciousness of its accompanying cognitive content; (*c*) whether the spot might not conceivably be following a co-conscious train of activity; (*d*) whether we should not have obtained highly emotional associations whatever part of the manifest content had been chosen for a launching-place.

An obvious comment upon our procedure is that the act of writing down the associations (since they could not be spoken) must have exerted a slowing, obstructing, and to a certain extent, directive influence upon the associations. We believe this to be true of some subjects. But the matter is not quite so simple as this. One subject, who had done a great deal of auto-analysis, during which all associations had been written down, found it easier to associate freely with a pencil in hand than when the ideas were expressed vocally. Still another subject reported this preference. Two others, more used to expressing themselves precisely in writing, found that feeling a pencil in their hand set up their professional habits of logical thinking, and would prefer to speak their free-associations if this were possible. We hope to try the effect of allowing the subject to use a dictaphone in a separate room. This may make matters easier for some, but it should be noted that by two at least of our subjects, writing is preferred.

Another difficulty was encountered by subjects who complained that their writing lagged behind their thought processes. They might think of a new subject several seconds before they were ready to write it down. This difficulty was probably more oppressive to those whose thinking was imaginal than to those thinking verbally. We hope to overcome or diminish it by the substitution of speech for writing.

We now come to the problem of registering the lapse of time during the experiment, in order to correlate the introspective records with the contemporaneous spot-deflexions. At first, the subject called out consecutive numbers to represent the main divisions of his flow of ideas. These numbers were then entered by the spot-recorder alongside the corresponding spot-positions. This procedure was abandoned at once. Its defects are many and obvious.

The procedure which has been employed in these preliminary experiments, but which we hope to improve, is that the experimenter in charge of the apparatus taps once at the end of every 10 seconds, and twice at the end of every minute. The subject makes one, or two, corresponding marks through his writing when the tap is heard. The spot-recorder reads the spot-position when every tap is given. He also records the minutes. He can usually follow any deflexions of the spot occurring during the 10 seconds, as under present conditions, the spot seldom makes more than two deflexions in the 10 seconds. Sudden deflexions, as distinct from gradual swings, are also noted by him, and subsequently recorded on graphs in a special way.

Improvements in this technique are possible[1]. The first is a more faithful means of recording the spot-deflexions. A photographic record is possible, and may eventually be used, but we are experimenting with the Brearley Spot Recorder, in which a pointer is kept congruent with the spot by an assistant who turns a highly-geared handle. The position is then recorded on a kymograph in the usual way. If the dictaphone proves practicable[2] a sound may be made electrically in its recorder contemporaneously with a mark on the kymograph, by means of a Brodie pendulum.

In spite, however, of the defects in the present technique, results which appear to be interesting and suggestive have been obtained.

III. Introductory Experiments.

Before embarking upon the actual process of dream analysis, we ought to have attempted, by means of preliminary experiments, to answer in advance the important questions whether the large spot-deviations occurring during dream analysis were not to be attributed primarily to changes of *attention*. If this were so, it might be that certain experiences were correlated with large and sudden deviations less because they were emotionally toned than because they involved intense attention or sudden changes in the direction of attention. To investigate this problem, the following experiments were subsequently devised to compare the psycho-galvanic phenomenon of subjects who were concentrating attention in various degrees upon tasks from which emotion was, as far as possible, completely excluded. These tasks were usually unlikely to bring up unanalysed dream experiences, which we believed to be likely to produce intense emotion.

Experiment 1.

The subject was asked to sit placidly and comfortably in the chair, to do nothing, and as far as possible to make his mind a blank. The two experimenters arranged a noiseless series of 10-second time signals behind the subject's back; so that from the subject's standpoint, nothing happened in the room at all during this passive period. Every 10 seconds the recorder read the spot's position, noting on his time record any

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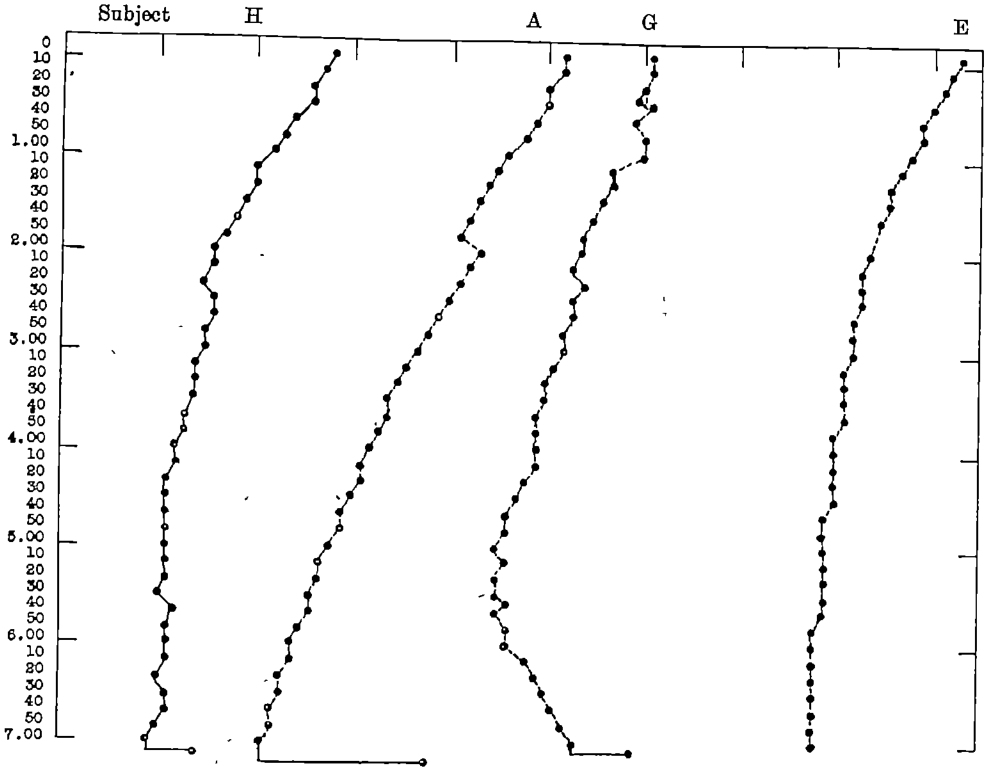
[1] We wish to acknowledge our indebtedness to Professor A. V. Hill for his kind help and valuable suggestions.

[2] Cf. W. Baade, "Über die Registrierung von Selbstbeobachtungen durch Diktierphonographe," *Ztsch. f. Psychol.* 1913, LXVI.



Initial resistance	70,000 ohms	20,000 ohms	12,000 ohms	27,000 ohms
Final resistance	100,000	30,000	14,000	40,000
Galvr. shunted by	5	20	10	5

Fig. 2. 'Passive' mental state.

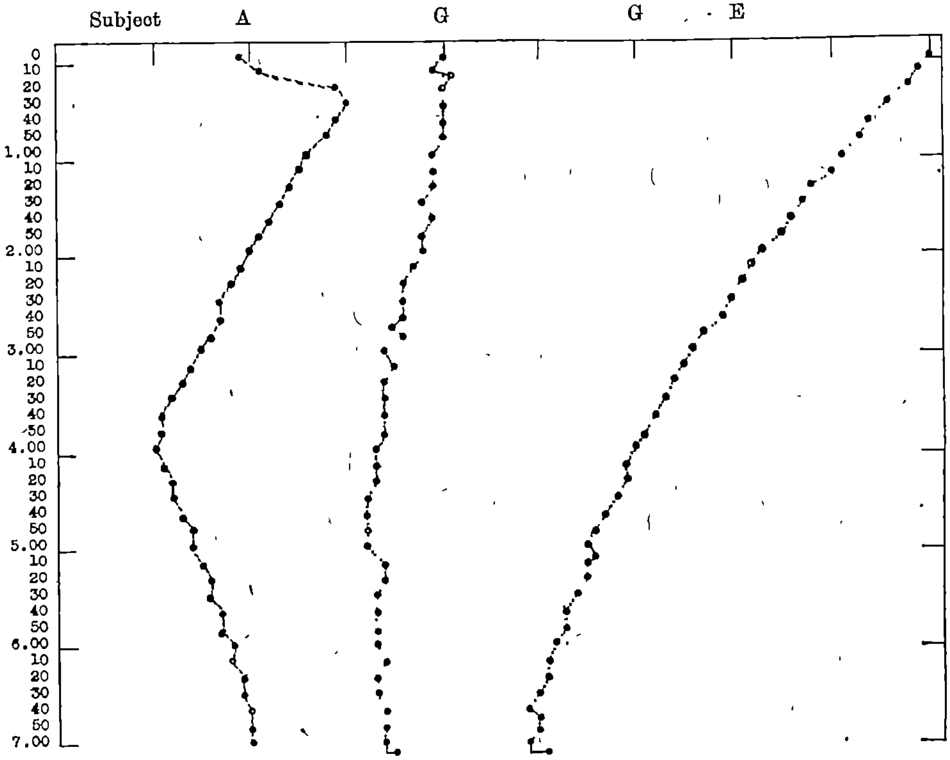
Note the similarity of the graphs, and their gradual 'drift' (due to physical changes in resistance). Emotion produces deflexions opposed to the drift. The horizontal line at the end of, and connected with, the graph always denotes the last deflexion, made when the subject became aware that the end of the experiment had come.

Subject G. said he realised that the end was coming, and the tapping caused slight emotion. E. said that his period "felt like a steady one." H. said that her so-called "passive mental state" was really a "series of repressions," *i.e.* she was trying to be passive.

accidental noises made outside the room. Such noises were few and seldom seemed to produce any great deflexions. The passive experience continued for 7 minutes. The length of the experiment was not told to the subject.

Experiment 2.

Immediately at the close of Experiment 1, the subject was given a pad of paper. He was instructed every time he heard a tap to make a vertical mark, and every time he heard two taps to make two such marks. He was asked to do and to think of nothing else if possible. This experiment also lasted for 7 minutes.



Initial resistance	35,000 ohms	22,000 ohms	30,000 ohms
Final resistance	35,000	29,000	67,000
Galvr. shunted by	20	10	2

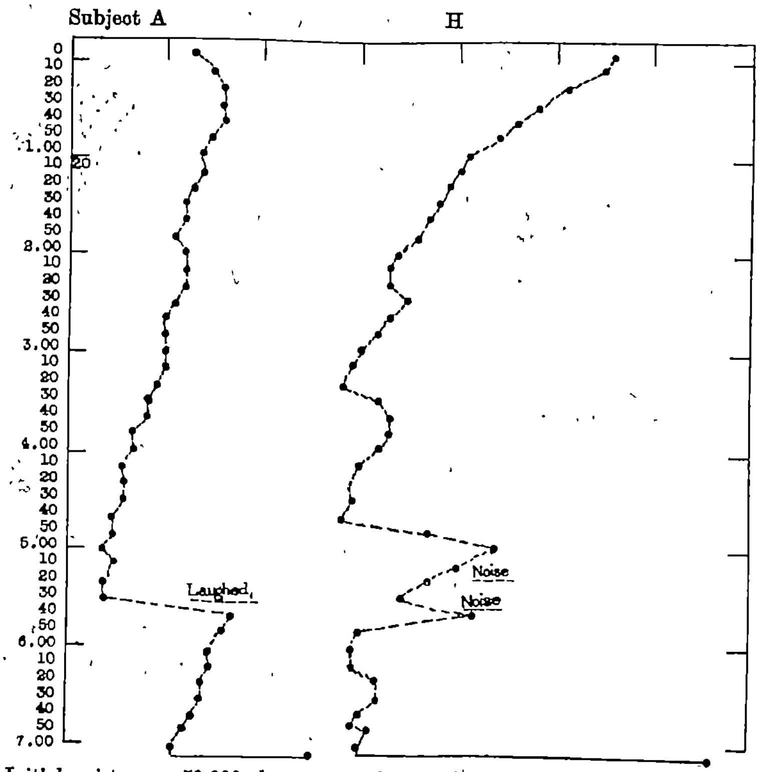
Fig. 3. Tapping.

No explanation of A.'s drift was found.

Experiment 3.

The instructions were the same as in Experiment 2, except that the subject was asked to write out again and again, at his ordinary rate of writing, the first verse of the National Anthem, making vertical time marks as before. (The experimenters previously had satisfied themselves that the subjects knew the National Anthem by heart.)

In this way it was hoped to give the subject a task which would employ attention at a constant level of concentration and yet would not be difficult. The experiment lasted 7 minutes.



Initial resistance 50,000 ohms 20,000 ohms
Final resistance 18,000
Galvr. shunted by 20 5

Fig. 4. Writing.

Note the emotional deflexions.

Experiments 4 and 5 could not be done on the same day as Experiment 3.

Experiment 4.

The subject was told that he would be asked to multiply, 'in his head,' two digits by one digit, and to write down the answer. He was directed not to use movements of his pencil, either in the air or on the paper, to help him in his calculation, but simply to write down the answer. Noiseless time signals were given by a third experimenter, who touched the spot-recorder's hand every 10 seconds and lighted a red flash lamp (visible to the apparatus-experimenter but not to the subject) every minute.

This experiment was intended to give the subject a task of greater difficulty than those given before; one which would require concentration of attention, but one which was well within his powers. No rests were given between sums, the writing down of one sum being the signal for the dictation of the next. The experiment lasted for 5 minutes.

Experiment 5.

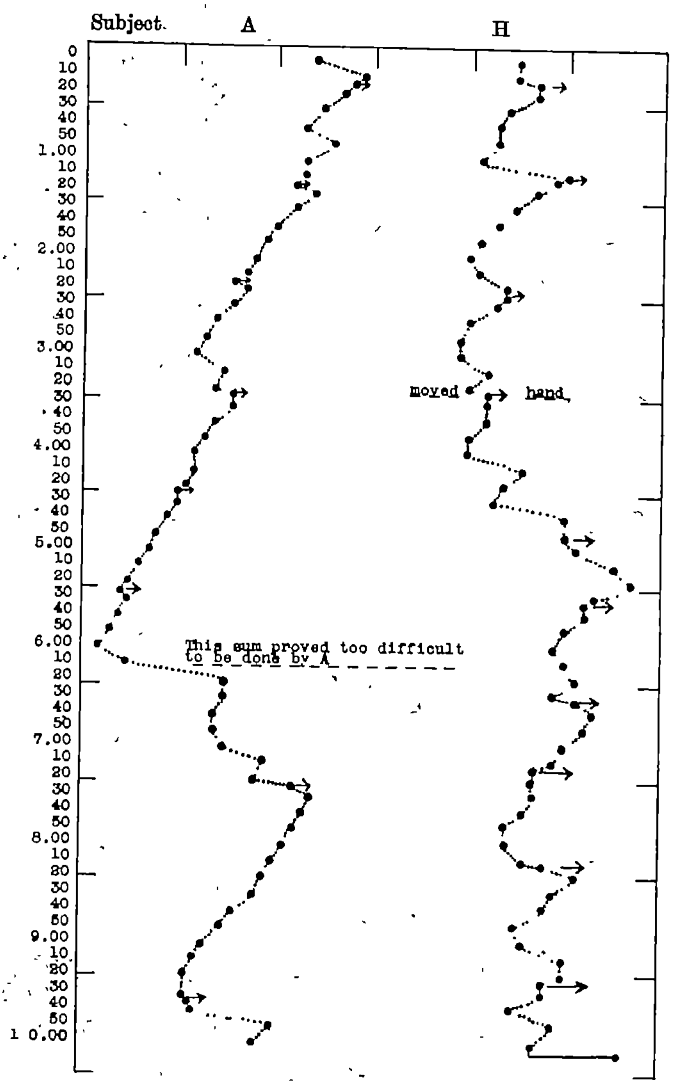
Instructions were given as in Experiment 4, with the following modifications. The subject was asked to multiply 'in his head' two digits by two digits. He was told that a sum would be given him at the end of a certain period of time (unknown to him) whether he had completed the sum on which he was working or not. If he had finished his sum before the next one was dictated, he was asked to rest mentally without changing his bodily position. The experimenter who dictated the sum raised his hand quietly as soon as he saw the answer being written down, and the spot-recorder noted the deflexion at that moment as well as at every 10 seconds.

The sums were actually given each minute for 10 minutes. They varied in difficulty, as in some of them both subjects used short cuts, but both of them found this part of the work difficult.

IV. The Deflexions obtained during Dream Analysis.

For several reasons, which to psychologists will be obvious, the dream-records published here are selected. They have been obtained from five subjects. They represent (*a*) the manifest content of the dream, (*b*) free-associations to certain parts of the manifest content.

A few words of explanation should make the records easy to read. Where no words are printed opposite a set of deflexions, it means that associations, though recorded, have not been reproduced in the diagram. On rare occasions it signifies that the timing arrangements failed. The word 'blank' opposite a series of deflexions means that nothing was written by the subject during this period. Printed words, not underlined, indicate that the 10 seconds division of time has been carried out successfully during the period, and recorded on the diagram.

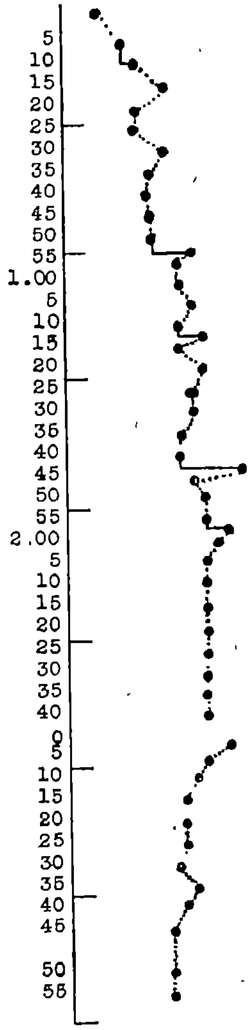


Initial resistance	25,000 ohms	21,000 ohms
Final resistance	23,000	20,000
Galvr. shunted by	5	5

Fig. 5. Multiplication. 2 digits by 2 digits.

The small arrows indicate the points at which the answers were written down. Note A.'s deviation between 6.00 and 6.20.

Underlining the associations by a continuous line means that they occurred during the *minute* indicated on the graph, but that the subject made too few or too many 10 seconds marks in that time, so that the associations cannot be allotted safely to any particular point on the curve. A note by the experimenter is underlined by a broken line.



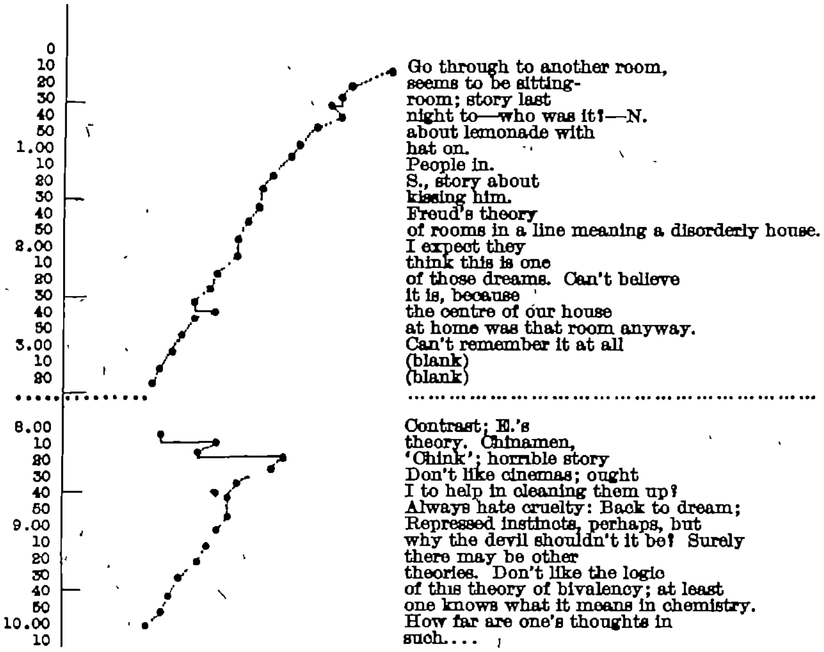
At home; L.
with face livid.
Go through to
another room.
E. very
stern;
S. with set face.
Has been
punishing dog
(Jack?) with
coal-hammer
for some fault.
Hammer with
point. On tem-
ples of head
and splintering the
bone; saw him
in dream.
Someone comes in
and (I think) I
try to appear
unconcerned while
determining
to make it warm
for him. Coal
hammer with a
point. Something
about dinner
and a caller. Tell S.
he has
been known before
to be cruel.
DREAM RECITED.
At home; L. with face livid.
Go through to another room. E. very
stern; S. with set face.
Has been punishing dog (Jack?)
with coal-hammer for some fault.
Hammer with point. On temples of head
and splintering the bone; saw him in dream.
Someone comes in and (I think) I try
to appear unconcerned while determining to
make it warm for him.
Coal-hammer with a point. Something about dinner
and a caller. Tell S. he has been known
before to be cruel.

Fig. 6. Dream written down. Subject E.

It may forestall certain questions to mention here that no one dream has yet been thoroughly probed by this method[1], and that the 'depth' or 'ultimateness' of the free-associations shown are not very great. In most cases, however, they produced emotion, which was the result desired.

Fig. 6 shows the complete record of the deflexions accompanying the writing-down of the manifest content of a dream[2].

The subject was then asked to associate freely from the item in the dream *Go through to another room.* The result is shown in Fig. 7.



Go through to another room,
seems to be sitting-
room; story last
night to—who was it?—N.
about lemonade with
hat on.
People in.
S., story about
kissing him.
Freud's theory
of rooms in a line meaning a disorderly house.
I expect they
think this is one
of those dreams. Can't believe
it is, because
the centre of our house
at home was that room anyway.
Can't remember it at all
(blank)
(blank)

……

Contrast; E.'s
theory. Chinamen,
'Chink'; horrible story
Don't like cinemas; ought
I to help in cleaning them up?
Always hate cruelty: Back to dream;
Repressed instincts, perhaps, but
why the devil shouldn't it be? Surely
there may be other
theories. Don't like the logic
of this theory of bivalency; at least
one knows what it means in chemistry.
How far are one's thoughts in
such....

Fig. 7. Free association. Subject E.
To "go through to another room."

Notes on Fig. 7. The following notes were made by subject E., after plotting the curves and considering their relation both to his recorded free-associations and to the memory-material behind them which is accessible to immediate introspection. It will be convenient to indicate the associations under discussion by the time-numbers opposite them.

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[1] This means simply that in every case there is reason to believe that other significant associations could be obtained if the analysis were pursued further; apart, of course, from the theoretical possibility that no dream is ever completely analysed.

[2] In this and the following diagram the spot-recorder has represented sudden deviations ('kicks') by a horizontal line. A mechanical method of recording would obviously represent exactly the rate at which the spot moved. Failing such an apparatus, this seemed to be a useful convention.

1.40 to 2.50. The subject-matter of this thought is obviously of a sexual nature, but 'intellectualised' by the frequent discussion of such matters in connexion with general psychological theory.

8.00. 'Chinamen' is aroused in connexion with the question of psychological contrast. A discussion of this problem which E. remembers, mentions that to an Englishman who has lived for years amongst Chinamen, his own countrymen, on his return to England, appear unusually tall. 'Chink' suggests Thomas Burke's story "The Chink and the Child," which E. has only read rapidly, but he has heard much of the effects of its cinematographic representation ("Broken Blossoms") upon children. The psychologically interesting feature of this record is the rapid decrease of the deviations when this subject is discussed in psychological terms, or rationalised. A further example of this is given in Fig. 8.

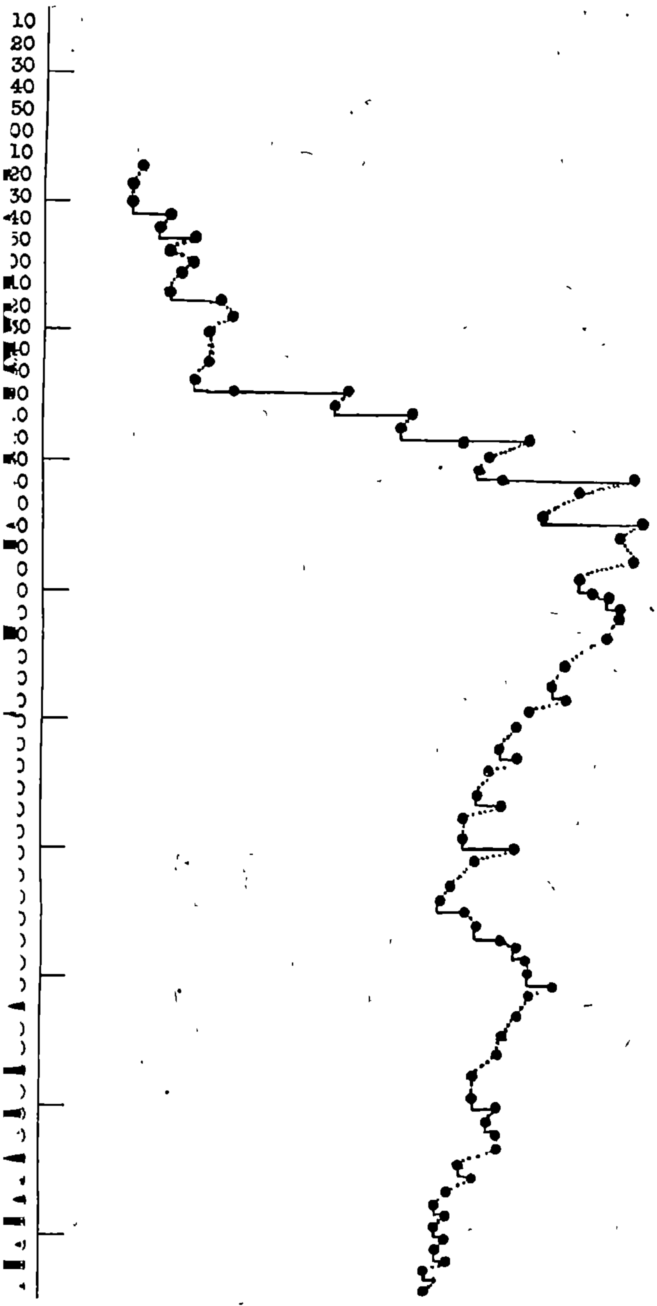
Notes on Fig. 8. The first associations are omitted.

This curve is of particular interest because in the middle of it there is represented a very considerable emotional disturbance. Its physiological concomitants, nausea and faintness, almost caused the experiment to be interrupted; the subject, however, deciding to carry out his instructions. It deals with a conflict (the unpleasantness versus the justifiability of vivisection) the solution of which can be reached only by reasoning. On former occasions, this conflict has often been lessened by such reasoning, and it is interesting and, perhaps, clinically important, to see how, when this dispersive force is brought into action, the galvanometer deviations almost cease.

2.20. E. returns voluntarily to consider the manifest content of the dream. A subject who was dishonest would probably not do so. At this point E. had a vivid visual image in the dream, and refers to it here.

2.50 to 3.00. At this point E. remembered a real experience: when at school he had accidentally kicked the shin of another boy instead of the football at which he was aiming. The cut was deep; and something white, which at the time he took to be the bone, was exposed. Note that the image which is connected with this deflexion is of a real experience, and is not transformed or distorted by the dream-work.

3.10. The notes mean "I don't know what this reference to 'temples' in the dream means." This is followed by an actual feeling of sickness. It should be noted that just before this the 'shin-bone' incident appears.



T. shunted by 20.

...Would like to hear
what A.G. says. A man who
beat a dog against a wall.
My fury. But it threatened him.
What would I do if a dog
threatened my children? Not
beat it against a wall, anyway.
I saw
the scene. Splintered temples.
Why splintered bone? I saw it so
clearly. Little R. at school.
I saw his splintered bone.
Temples; I don't see
what I feel
just the

sort
of way I would in R.'s lab, that
Sunday when A. took me. Cats
and G.'s description, and M.
saying he couldn't sleep. But it
must be done, so far as I can see,
and I am glad enough to accept
............[1] This is wrong;
I meant, to accept the vicarious
responsibility for it all. One thing
that strikes me is that R. and G. are
such decent chaps. But there is X.
Does he really hate it all? No,
I can't believe that, and I thought
of him early. He
is S., and so is Uncle. Uncle's dog;
kept it in, and so it
was dangerous; I think Father
said it was cruel. K.Q. and his dog.
A game yesterday; the E.'s;
D.C. and masochism
and our joke.

but I want to know
more about all this. Still think McDougall
with his broad gauge is
better. H. and A.;
but it was K. seen last
night. Shaw and his views.
I don't like those dogs
either; barking in the lab. I feel
I am doing the best I know.
but I don't like it. The trouble is the anti-vivisectionists
I know are such 'softs'; at least, I think so.
Aunt D. and her dog; sent in a box. Made me
annoyed the other day at Stafford when

Fig. 8. Free association from 'coal-hammer.' Subject E.

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[1] The word responsible was written here but crossed out.

Whether, by a kind of congruity of affect, the sickness now makes E. think of another actual experience (at 4.00) which was nauseating, it is impossible to say.

3.30 to 4.00. E.'s notes point out that though the time-intervals and the words corresponding to them suggest that he was thinking at this slowed rate, he cannot say, retrospectively, whether this was so or not, as at the time he was really feeling sick and faint, and thought he might actually be unable to continue writing. But, he writes "even if visual images had been hurtling through my mind, I should not have been likely to record them in this state. I cannot say whether a person who thinks in words would have been able to do this or not."

4.20. (From here onwards E.'s notes are quoted verbatim.)

"Here there enters the most potent aid to rationalisation that I could have wished for, had I been trying to suppress the affect; a memory of one of the most brilliant exponents of vivisection expounding the details of a very thorough-going experiment of this kind. If the free-associations and the curves really fit; *i.e.* if the time-lag between the moment at which the idea enters consciousness and that of the spot-deviation, is equal to that between the entry of the idea (or image) and the act of writing which expresses it, the following interesting conclusions follow:

"(*a*) 4.20 to 4.30. Actual memory of a distinguished philosopher telling me that hearing the above description would cost him a night's sleep. At the time, this discovery of someone else's difficulties was reassuring to me.

"(*b*) 4.40. Note the emphatic expression of my own opinion, probably very much strengthened by the memory of the above exposition. Is this not strengthening my resolution by mentally calling up the leaders of my community (or herd)?

"(*c*) 5.00. Note the mistake crossed out; the emendation was very significant at the time, and alters the moral significance of the whole problem. But the 'kick' might relate to the 'R & G' theme.

"5.40. I dislike X., and always feel dislike when I think of him. One of the chief reasons which I consciously give to myself, and a very real one, even if psycho-analysis might show it to be relatively superficial and not the deepest one, is given at 5.50 to 6.00. It is cruelty to animals.

"6.20. One external and several internal similarities seen between him and uncle.

"6.20 to 7.00. A great complication of subjects here; all connected with the theme of the dream. Memory of actual dog belonging to uncle;

K.Q., was once very cruel to a dog. He plays a certain game very well; uncle was the first good player of the game whom I knew. They are alike in build. I have this good reason to dislike K.Q., and at least two bad reasons; he has aroused the inferiority-feeling in me in two ways, one much more potent than the other.

"7.00 to 7.20. Amusement about a lecture by a well-known critic who 'guyed' the masochistic novels of a certain 'best seller.' A little joke with the E.'s arising out of this.

"7.30. My impression is that from 7.30 to 8.10 I was thinking about non-emotional things; the 'broad gauge' on which McDougall's *Outline of Psychology* seems to me to run.

"9.30. Here if the experimenters hadn't interrupted me, I should probably have got back to the central topic of the dream."

A general note by E. is "my curves suggest that I can apparently dispel the emotional disturbance by thinking about the exciting incident in relation to other subjects in connexion with which I have powerful and integrated sentiments[1]."

It may be remarked here that though this seems to be orthodox psychology, it is interesting to see an apparent confirmation of it by this objective record.

V. RESULTS OF THE PRESENT INVESTIGATION.

Whatever contributions a continuance of this investigation may make to theory, it has been successful in one practical detail. An obvious defect of most experimental investigations of emotions by the method of expression (perhaps of all laboratory work upon the emotions) has been the difficulty of producing intense, personal poignant emotion. Work on the psycho-galvanic phenomenon is still so recent that criticism of its insufficiency is scarcely fair. But the emotional changes which have been investigated hitherto have often been produced by causes comparable to the practical joke. The subject is startled by sounds, by pin pricks, or by threats of these stimuli. But very soon many subjects become almost immune to these violences and to threats of them. Or emotion is aroused by talking about the subject to others in his presence. This requires—for two reasons—that one should know him very well.

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[1] E.'s notes suggest that lessening deflexions may be attributed to rational thinking about the emotional subject, yet possibly they are due to a merely physical cause—the decreased tendency to show the psycho-galvanic reaction after a large deflexion. At present, there appears to be no evidence that a sustained emotion would produce a continuous series of deflexions.

The word-association method has produced interesting results, but seldom helps one to penetrate into the depths of a personality. It is not the dynamite charge of the miner, but the blunderbuss filled with spreading shot. Most of it misses its intended victim. A little penetrates, but not far. And even of those pellets which make him wince, some have only too obviously struck him on parts which are tender in most people.

The use of unanalysed dream material as a potential provoker of emotion seems to be less open to this kind of objection. Even those investigators of dreams who do not subscribe to the belief that the dream *never* troubles itself with trifling things, will probably admit that it commonly does not. It is generally admitted that the dream has a personal significance. And experience in dream analysis makes it certain that, in intellectually honest persons, the associative material aroused in connexion with the manifest content of most dreams, produces emotion which is intense, complex and intimately connected with the person's past and present life. Possibly, therefore, whatever may be learnt about dreams by the use of this method, something may be discovered about their relation to emotions, sentiments and complexes.

We believe, also, that some results which cannot be reported here, may cast light upon the relation between the intensity of affect in the manifest dream and the amount of transformation which the latent content has undergone. We have seen physiological deflexions which appear to be the result of 'pulling oneself together,' when emotion threatens to overcome one; interesting examples being those in which the concrete emotional experience is thought of in a wider context, and relate to more general experiences about which there are strong and integrated sentiments.

Another record of dream analysis suggested that the method might conceivably make clearer the various differences between those two kinds of memories which one of us has distinguished as repressed and superseded[1], or exiled memories and 'dead selves.' It was interesting in one dream analysis dealing with memories of about 15 years ago, to notice the memories which still had power to swing the ray of light and those which apparently had lost it. Two dreams from another subject, of 14 years and 8 years ago respectively, show the same difference. Some day an improved psycho-galvanic method may be useful to the psychotherapist, who would be glad to know whether memories, which at the beginning of a course of treatment disturbed the patient, can at the end of it be thought about without emotion. In this or in a similar way, light may possibly be cast upon the conflicting claims of different methods of psychotherapy to produce lasting effects, and upon the procedure of a physician who produces transference and fails to dissolve it.

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[1] *Remembering and Forgetting*, pp. 166–76.

A further point for future research suggested by the present series of observations was the possibility of two kinds of mental blank when the subject was unable to continue free-association. First there may be blanks due to repression, indicated by a deflexion following the failure to continue free-association, and secondly blanks due to genuine absence of cognitive content, indicated by absence of such deflexion. Further research directed towards the elucidation of this problem will be necessary before any opinion can be expressed as to whether this is a genuine difference.

It must be admitted that at this stage of the investigation many problems have been raised and very few solved. No answers are, for example, yet forthcoming to the questions raised on p. 24. Even, however, at this early stage, there are indications that this research may pave the way to solving some of these and similar problems. Nothing can be more crippling to the beginnings of any research than a perpetual desire to be immediately useful, to practice or to theory. The mere collection of facts in a scarcely investigated field may be of more value than a premature attempt to reach conclusions.

COMPARISON OF CERTAIN INTELLIGENCE SCALES

By J. H. WILSON.

I. Introductory (pp. 44–46).
II. Norms, time elements, cost (pp. 46–47).
III. Comparison of total scores (pp. 47–54).
IV. Analysis of the several tests in the scales (pp. 55–62).
V. General comparison of the scales (pp. 62–63).

I. INTRODUCTORY.

THE object of the experiment described in the following pages was to evaluate the worth of the following five scales:

1. Terman Group Test of Mental Ability.
2. Otis Group Intelligence Scale. Adv. Exam. A.
3. National Intelligence Tests, Scale A 2.
4. Northumberland Mental Tests, I.
5. Simplex Intelligence Scale.

Subjects and Administration. Approximately 340 children were tested with the first four scales mentioned and, as far as possible, the pupils took all the tests. The children were taken from four schools. Of two, all who were within the age limits, were tested. In the case of the other schools, the ratio of the numbers selected from each was, as nearly as could be managed, that of the total numbers on the rolls. From within each school the children were then selected at random by taking them in the alphabetical order of their names. Three of the schools were elementary, and the fourth was a girls' boarding school, the pupils of which were the daughters of professional gentlemen. Of the elementary schools the pupils of one were considered by the L.E.A. to be of very poor social standing, while those of the other two were considered to be, if anything, above the average.

The tests were given towards the end of June and beginning of July 1922. The order in which they were given in any one school was purposely made different from that in which they were given in any other so that the influence of familiarity with test procedure and of practice might affect all scales equally.

The Simplex Scale was given to the pupils of one school only; and, to include a variety of ages, they were selected according to the alphabetical order of their names. The full influence of any practice effect due to the survey was, in consequence, received in maximum measure by it.

All the scales were administered and marked by the writer and that the children might not be embarrassed by strange surroundings, the scales were given in their own schools; and the teachers kindly helped with the invigilating and supplied the supplementary data collected.

The Terman Group Scale is designed primarily for pupils in the upper standards of the elementary school. The pupils examined ranged in age from eight years to thirteen and eleven months.

The Otis Group Scale is designed to suit a range somewhat similar to that of the Terman and was given (as far as circumstances of illness, school organisation and the like permitted) to the same children.

The National Scale A 2 is planned for use with children from eight years to thirteen years.

Table I.

Scale		Age						
		8–0 8–11	9–0 9–11	10–0 10–11	11–0 11–11	12–0 12–11	13–0 13–11	
Terman	Average	26·3	27·0	54·0	63·1	84·4	94·2	
	St. Dev.	14·4	17·9	23·9	32·1	35·1	37·9	
	Median	30	23	53	60	84	91	
	No. of pupils	7	11	82	91	94	48	333
Otis	Average	35·0	64·0	81·5	89·7	108·2	115·7	
	St. Dev.	15·1	26·6	26·8	30·3	31·4	32·7	
	Median	36	53	80	90	111	112	
	No. of pupils	6	5	83	98	102	44	338
National	Average	43·7	67·0	84·2	96·0	114·3	121·3	
	St. Dev.	22·5	23·1	24·8	28·9	25·9	27·5	
	Median	45	58	86	96	118	121	
	No. of pupils	8	8	83	95	97	48	339
Northumberland	Average	—	—	22·5	25·6	34·5	38·0	
	St. Dev.	—	—	12·6	14·0	13·7	15·8	
	Median	—	—	19	23	35	42	
	No. of pupils	—	—	91	108	112	30	343
Simplex	Average	—	—	68·5	66·7	74·5	82·7	
	St. Dev.	—	—	18·3	18·1	25·6	26·4	
	Median	—	—	52	66·5	84	85	
	No. of pupils	—	—	6	26	29	17	78

The Northumberland Scale is planned to suit children between the ages of ten and twelve and a half. This scale and the National were given to children older than the specified limits. It was hoped to see to what extent these scales could be used in these higher reaches.

The Simplex Scale was given to all the pupils in one school from ten years of age to thirteen and eleven months. The scale is intended for use with pupils even older than this.

A general idea of the results obtained may be gathered from the central tendencies and variabilities given in Table I. It will be seen that but few pupils below ten were tested. The results for such children though included in the table are not used in the following analyses.

II. Norms. Time Elements. Cost.

A. Table A gives the *norms* published with the various scales. For the Terman the numbers 27, 47, 67 and 87 may be used tentatively[1].

Table A.

Scale	10 yrs 6 m.	11 yrs. 6 m.	12 yrs. 6 m.	13 yrs. 6 m.
Terman	No norms published			
Otis	70	82	94	106
National	75	90	101	112
Northumberland	15	24	33	42
Simplex	33	46	59	74

The Otis Scale has norms which have been made on the results of a very wide range of testing in America. The numbers involved are over 25,000 and are from 200 cities scattered throughout that country. Furthermore the distributions of scores of the pupils in the various grades tested were augmented so as to adjust the proportion to that actually existing in the schools. The norms should be very valuable. As far as our results can be said to represent the scores of British children, it would appear that the American norms are too low, for in each of the four years compared are significant differences.

The same may be said of the National Scale A. In this case, however, the norms are themselves only tentative, and no indication is given of the numbers of pupils tested in order to establish them. The differences in the successive years are more marked than in the case of the Otis.

Comparison with the Northumberland results would show that our children are average except for year ten to ten and eleven months. This scale has been standardised upon 2532 English children and in consequence it is quite probable that our age ten to ten eleven group is somewhat above the average for such children. This does not invalidate the higher scores obtained in the American Scales in the other years. As the Northumberland Scale is not framed for use with more than twelve years six months, we cannot deduce anything from the discrepancy found between our result for thirteen six and the given norm.

Were the Simplex norms other than purely tentative ones, it would appear that our children were above normal. As it is, no such conclusion would be safe. Over and above this our numbers for this scale are small.

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[1] Calculated from a formula of Mr C. A. Richardson, *vide* his "Mental Tests."

We must conclude, therefore, from our results that the norms as supplied by the authors of these scales are most satisfactory in the case of the Northumberland.

B. In giving and in scoring the tests time was economised as much as possible. The average times taken by us to administer the scales were for the Terman 50 minutes, for the Otis 75, for the National 45, for the Northumberland 75, and for the Simplex 95. The American scales were more difficult to administer than the British.

Our rate of scoring may interest those about to make a survey. Taking averages based on the marking of our booklets we have ten Terman, nine Otis, ten National, twenty Northumberland, and seven Simplex scored in the hour.

C. There is very little difference in the cost. Northumberland and Otis are cheaper than the National and Terman.

III. COMPARISON OF TOTAL SCORES.

A. Distributing the scores by (*a*) Ages, (*b*) Standards, gave approximately normal results within each of these limits. There were extremely few zero scores, there being but one in Terman, less than four per cent. in Northumberland, none in either the Otis, the National or the Simplex. The Northumberland was the only one in which the maximum score was reached.

B. The correlations between examination marks[1] and the intelligence scores are shown in Table II. It will be seen that these results apply only to the elementary schools. The data was collected for the other school but is not presented here as the analysis to be made deals only with the elementary school results. As the average value for the correlation coefficient is ·46, it would appear that the scales reveal a reasonably high correspondence with the examination findings, for while the manner in which the results for the girls' school were collected tends to lower the correlation, the average is as high as is generally obtained (Terman ·44, Holley ·46). We have failed to find any definite theoretical proof that such coefficients should lie between the limits ·40 and ·60, if the scales are to be satisfactory. This assumption seems nevertheless to be customary.

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[1] The examination marks awarded in the different schools, differ somewhat in the way in which they were assembled. Those for the girls' school, M.S.G., include marks awarded during term time and also marks for good conduct and attention. Those for the boys' school M.S.B., and for the mixed school S.P., are awarded only on the examination of the ordinary school subjects. This may account to some extent for the rather startling differences discoverable in the Table. It does not account, however, for the persistently high coefficients got with the National Scale in both cases. The influence is more pronounced with St. V than with any of the other classes. That this teacher had attached much importance to considerations other than marks gained in the academical subjects, is shown by examination of her list of scholars. To take a single instance, we find that the child who is placed third in order of merit is estimated as having "*d*" intelligence on a rating "*a*,' "*b*," "*c*," "*d*," "*e*," while under "Remarks" is added "Good worker."

Table II.

School	Stand.	Scale				
		Terman	Otis	National	Northumberland	Simplex
M.S.G.	III	·37	·41	·47	·64	—
	IV	·58	·37	·46	·35	—
	V	·09	neg.	·54	·24	—
	VI	·73	·51	·58	·47	—
M.S.B.	III	·69	·63	·63	·54	—
	IV	·54	·52	·42	·47	·68
	V	·68	·50	·54	·44	·18
	VI	·57	·23	·41	·23	·49
S.P.M.	IV	·26	·41	·12	·12	—
	V	·54	·46	·34	·74	—
	VI	·65	·50	·44	·51	—

The probable errors to these coefficients lie between ±·07 and ±·15. Bravais-Pearson formula.

The tests in order of correspondence with examination marks, rank as follows: Terman ·49, Simplex ·43, Northumberland ·42, National ·39 and Otis ·39. The National shows a fair amount of correspondence with marks estimated over a whole term, more in fact than any of the other scales.

Table B gives the distributions of the coefficients of correlation between intelligence scores and examination marks.

Table B. *Distributions.*

Coefficients	Number
·70......·79	1
·60......·69	3
·50......·59	10
·40......·49	9
·30......·39	3
·20......·29	4
·10......·19	3
·00......·09	2

C. The following Table (III) gives the distributions of the teachers' estimates of intelligence. The teachers were asked to rate the pupils on a special sheet, according to instructions which were those usually quoted when a five letter rating is required. In a normal class 5 per cent. were to be *A*'s, 20 per cent. *B*'s, 50 per cent. *C*'s, 20 per cent. *D*'s, and 5 per cent. *E*'s.

Table III.

Standard	Rating	No. of ratings as per letter at sch.			Ratings all sch.	
		1. M.S.G.	2. M.S.B.	3. S.P.M.	No. of pupils	%
II	A	0	0	0	0	0
	B	0	0	0	1	7
	C	0	3	0	3	22
	D	2	5	3	10	71
	E	0	0	0	0	0
III	A	0	0	0	0	0
	B	0	5	4	9	21
	C	6	9	5	20	45
	D	7	4	3	14	32
	E	1	0	0	1	2
IV	A	0	5	0	5	6
	B	4	12	9	25	28
	C	21	15	7	43	47
	D	12	2	3	17	18
	E	0	1	0	1	1
V	A	0	5	0	5	6
	B	6	15	7	28	32
	C	16	10	12	38	44
	D	9	3	4	16	18
	E	0	0	0	0	0
VI	A	3	3	0	6	9
	B	4	21	12	37	57
	C	8	6	6	20	31
	D	1	0	1	2	3
	E	0	0	0	0	0
VII	A	2	1	0	3	12
	B	1	5	9	15	60
	C	2	3	1	6	24
	D	0	1	0	1	4
	E	0	0	0	0	0

It will be seen that the teachers were not able to follow these instructions very closely, yet the figures for all the schools show surprisingly close agreement with that to be expected theoretically. The higher standards show the greater divergences.

The coefficients are collected in the following Table C.

Table C.

Standard	Terman	Otis	Nat. A	Northumberland	No. of pupils
III	·43	·43	·48	·26	33
IV	·52	·20	·44	·20	62
V	·31	·24	·44	·37	57
VI	·31	·48	·18	·31	45
VII	·14	·17	·05	·14	28
Averages ...	·34	·30	·32	·26	

Grand average ... ·30
Bravais-Pearson formula.

These results are somewhat lower than those obtained with scholarship as judged by examination marks. They seem small enough to allow of the conclusion[1] that the scales do not depend largely upon schooling. Results, however, obtained by Jordan[2] gave Otis ·73, and Terman ·66.

D. Table IV gives the coefficients of correlation[3] between the scales. In order to have the subjects as homogeneous as possible the coefficients were calculated from the scores of each year group or standard.

Table IV.

	Terman with				Otis with			Nat. with		North.	
Group	Otis	Nat.	North.	Simplex	Nat.	North.	Simplex	North.	Simplex	Simplex	No. of Pupils*
10–0 10–11	·93	·62	·73	—	·85	·86	—	·72	—	—	87
11–0 11–11	·81	·75	·62	·79	·90	·78	·77	·72	·46	·82	89
12–0 12–11	·90	·82	·86	·92	·89	·85	·93	·78	·80	·83	102
13–0 13–11	·87	·86	·86	·90	·83	·82	·93	·86	·83	—	44
Av. ...	·88	·76	·77	·87	·87	·83	·88	·77	·70	·83	
Stand											
III	·89	·78	·76	—	·84	·53	—	·75	—	—	33
IV	·75	·77	·64	·88	·86	·60	·67	·61	·78	·92	62
V	·72	·60	·60	·77	·74	·68	·64	·46	·37	·68	57
VI	·78	·63	·68	·82	·65	·66	·72	·44	·47	·70	45
VII	·80	·85	·80	·41	·80	·74	·44	·63	·60	—	28
Av. ...	·80	·73	·70	·72	·78	·64	·62	·58	·56	·77	

* With the Simplex scale the numbers were for the year groups 26, 29 and 17 respectively and for the standards 17, 22, 27 and 10 respectively.

The number of pupils taking the Simplex Scale is small. All remarks made regarding this scale can be considered as tentative only and based upon inadequate data.

It will be noticed that the coefficients are remarkably high which would indicate that the scales are measuring much the same thing.

It will be observed that the Simplex correlates with the American scales much more highly than does the Northumberland, correlating most with the Otis and least with the National. The similarity of some of the tests of the Simplex to those of the American scales may have much to do with this. The Northumberland correlates more highly with the Otis than with either of the other American scales.

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[1] In drawing such a conclusion it is necessary to remember, that were potential scholarship and intelligence perfectly correlated, there would in practice always be cases of disparity between intelligence and scholarship grades. Hence any statement regarding the validity of a test as a predictive measure is premature or speculative until the disparity has been measured and the factors relating to the disparity ascertained.

[2] *Journal of Educational Psychology*, vol. xiv., no. 7, p. 422.

[3] The coefficients for Terman, Otis, National, and Northumberland were calculated by the Bravais-Pearson formula, while those for the Simplex were got by use of Spearman's 'footrule" and then corrected.

The British scales show a very high inter-connection while the Otis shows high correlation with all the others.

When the diverse character of these measuring scales is considered, the fact of such high correlation is somewhat remarkable. The conclusion that they measure much the same thing is strengthened by the fact that these correlations compare very favourably with the figures obtained by Otis for the two forms A and B of his scale and by other workers with the same material. Thus Otis[1] got values from ·74 to ·94, Snarr[2] ·79 and Colvin[3] ·83. Also Colvin[4] working with two halves of the Brown University Test, found correlations as high as ·76.

On the other hand other investigators using different scales have not found nearly as close an agreement. We might instance the findings of Geyer[5], using the Otis Scale and the Illinois Examination ·64; those of Clement[6] who secured Army-Otis ·57; Otis-Thurstone ·46; Otis-Pressey ·44; Otis-Indiana ·34; and finally those of Holley[7], Otis-Virginia (delta 1), ·68 and ·89.

For purposes of comparison we have gathered together results from other workers: for Otis with Terman ·80 (5), with National A ·76 (3), with National B ·73 (1); for Terman with National A ·85 (1), with National B ·76 (1). The numbers in the brackets indicate the numbers of "*r*'s" entering into the average.

E. Jordan[8] has calculated the number of cases in each fifth of one series, that would be in the corresponding fifth in a related series. Thus for correlation of ·50 it is 32 per cent. correctly placed, for ·70—40 per cent., and 57 per cent. when the correlation becomes ·90. It is seen therefore, that for reliable classification of pupils into groups of homogeneous ability, a correlation coefficient of ·90 and more is required. These facts point to the distance we are from tests that will accurately prophesy[9]. The accompanying Table V gives results from 55 pupils of

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[1] *Journ. of Educational Psych.* No. 12, May 1918, p. 237.

[2] *Ibid.* No. 13, Jan. 1922, p. 45.

[3] *Journ. of Educational Research*, vol. III, Jan. 1921, p. 1.

[4] *School and Society*, x. 27.

[5] *Journ. of Educational Psychology*, vol. XIII, No. 1, p. 43.

[6] *Educational Administration and Supervision*, No. 6, Nov. 1920, p. 433.

[7] *University of Illinois Bulletin*, vol. XVII, No. 28, March 1920.

[8] *Journ. of Educational Psychology*, vol. XIII, 1922, p. 419.

[9] If group tests were to be used for classifying pupils into small groups of homogeneous ability, we could expect apparently a great many mistakes, but the real significance of this would depend upon how far a given pupil is out of place, how serious for the purpose

Standard V, which afford some empirical evidence to compare with theoretical expectancy. Thus with coefficients from ·60 to ·70 we have 40 per cent. in their corresponding fifths.

Table V.

	No. of pupils displaced								
	by Otis			by National			by Northumberland		
Terman	One section	Two sections	Three sections	One section	Two sections	Three sections	One section	Two sections	Three sections
Sect. A	2	0	0	3	1	1	1	2	0
B	3	3	3	6	1	0	4	3	1
C	8	1	0	3	5	0	5	3	0
D	10	0	0	3	2	0	4	1	1
E	4	2	1	3	1	2	1	1	3
Total	27	6	4	18	10	3	15	10	5
%	50	11	7	33	18	6	28	18	9

Coefficients of correlation for this group are Terman-Otis ·72, Terman-National ·60, and Terman-Northumberland ·60 (Bravais-Pearson).

F. In the discussion which follows, the assumption will be made that there is a difference in intelligence level between the standards of an elementary school. Of course we have no reason for imagining the difference to be of equal amount. Quite possibly as higher standards are reached the differences are actually less, and in consequence, the intervals shown by a scale will probably not be equal. We do assume though, that the intervals are appreciable and that a good scale ought to reveal them clearly. One reason for the selection of a school-grade basis was that of convenience. Also such a basis has the advantage of giving more comparable groups.

With this assumption we seek the differences in intelligence scores between the different standards. That scale which reveals the most valuable differences between the averages for the different standards may be considered the most discriminative.

In the Binet-Simon Scale the procedure is to use an age basis. For comparative purposes either an age basis or a grade basis would serve.

in hand is such a displacement and upon how much better even such a classification is than the grouping which prevails. As a matter of fact, nothing is commoner in educational literature (vide *Administration and Supervision*, April 1920, p. 198; *School Review*, Feb. 1920 and May 1921) at present than favourable and even enthusiastic reports of experiments in classification on the basis of scores in some intelligence test. Disregarding the possibility that where the plan fails the experiment is not published, this would seem to show that great accuracy in ranking pupils is not essential, at any rate for a most noticeable improvement over present practice.

Since the different scales have different score units it follows that the differences cannot be compared as they stand. For this reason the probable errors of the differences were calculated. In order to obtain a comparable unit we found the quotient derived by dividing each difference by its probable error. This we may call the coefficient of variability. It is commonly held in statistical circles that a quantity should be at least three times its probable error, before it can be considered of any value.

Further an examination of the formula for the probable error of the difference between two averages will show that its magnitude depends upon the number of cases involved. In order therefore, that our results should be strictly comparable we selected for the analysis which follows *only those pupils who took all four scales.* (The Simplex must always stand apart on account of the small numbers involved.)

On doing this we found there were still left 239 pupils who were distributed among the classes as follows: 28 in St. VII, 45 in St. VI, 57 in St. V, 62 in St. IV, 33 in St. III, and 14 in St. II. The average age in St. VII was 12·11, in St. VI 12·7, in St. V 12·1, in St. IV 11·2, in St. III 10·6 and in St. II 8·10.

To facilitate examination the coefficients of variability are collected in Table VI.

Table VI.

Age	Terman	Otis	National	Northumberland	Simplex
9½—10½	6·63	2·12	2·92	—	—
10½—11½	3·21	2·90	4·37	2·42	—
11½—12½	6·43	6·23	6·83	7·06	—
12½—13½	2·20	2·11	2·36	1·69	—
Standard					
II—III	6·20	6·90	4·97	—	—
III—IV	6·27	8·57	7·66	4·84	—
IV—V	8·59	7·22	8·52	7·59	4·81
VI—VI	9·36	9·92	7·71	5·65	6·23
VI—VII	3·61	3·53	3·46	1·68	2·98

As the number of age groups is small we prefer to use the standards for comparative purposes. The results by ages however, show that there are differences in discriminative powers as we go from year to year and scale to scale. The reliable data would suggest as a tentative order for the scales, Otis, Terman, National, and Northumberland. None of the scales show any discriminative difference between our 12½ years group and 13½ years group.

For range of suitability the Terman would easily rank first, while the range for the Northumberland would appear somewhat restricted.

Returning to the standards, there too some of the scales are more discriminative than others.

The Northumberland is obviously limited to standards below the sixth. It discriminates well between the third, fourth, fifth, and sixth, though not nearly as well as do the American tests. It is not fine enough as a measuring instrument to separate our St. VI from our St. VII, which the American tests do with considerable success.

About the Simplex Scale, little may be said. Strictly speaking the results are not comparable. However, the scale is very successful with the standards tested by it. Comparing it with the Northumberland, even under its severe handicap, it comes out creditably. Its unit is fine enough to give a much higher discriminative value to the level VI to VII than does the Northumberland. Taking the fewness of the pupils into consideration, the scale would appear to compare favourably with the American tests. The difference in the numbers of examinees, of course, prevents a definite conclusion being reached.

The range of the National was purposely limited to give a fine unit, it being recognised that a task to suit senior pupils would be too stiff in content or in timing to suit lower grades. Excepting the Northumberland for a moment, the other scales would appear to have achieved a similar success.

Of the American scales there is little to help choice between them. Each shows reliable discriminative powers throughout the whole range tested. They are capable of discriminating between the pupils of St. VI and St. VII, at which level the Northumberland failed, and for the other standards show astonishingly high coefficients of variability. If we consider the averages of their coefficients Otis ranks first with 7·23, Terman second with 6·81 and National third with 6·46.

However, from a theoretical point of view the differences shown between the discriminative abilities of the separate scales at each level is much more interesting. There are noticeable differences in each row. Ignoring the Simplex results, we see that the Otis has the greatest discriminative power at level II to III, again at III to IV, and once more at V to VI. On the other hand the Terman has it at levels IV to V, and VI to VII, though the National is a very close follower at IV to V. Remembering that the figures are strictly comparable these facts imply, not only that some scales are more discriminative than others at certain levels, but also that we *may secure better scales than we now have.*

Coefficients for a wider range may be found. We present those for the range St. III to St. VI: Otis 24·2, Terman 20·1, National 19·4, North 18·0.

IV. ANALYSIS OF THE SEVERAL TESTS IN THE SCALES.

A. The five scales we have used are each composed of several series of questions or things to do. For convenience of description we shall endeavour to use the term "scale" to denote the instrument as a whole and the term "test" to denote each separately organised group of questions or things to do. Thus the Terman Group Test of Mental Ability consists of ten tests, the National Intelligence Tests, Scale A Form 2, of five, the Northumberland Mental Tests, No. 1, of twelve and the Simplex Group Intelligence Scale contains twenty-six.

The scales deal with a large number of tests which probably are of different values. In order to determine the extent of any such differences, distributions were made for each test by standards.

Marked changes in the curves of distribution are shown by each test in passing from standard to standard. Differences, too, are shown by the several tests throughout all standards, while the distributions of the tests of the same kind occurring in different scales also exhibit marked variations. These facts would indicate that certain tests are not suitable for some of the standards, and that altering the test material influences considerably the range of suitability of a test.

In order to make these differences still clearer, we have treated the test scores in the same manner as the scale scores. The results are presented in Table VII.

The first thing that strikes the attention is the presence of inversions (*i.e.* cases where the score in a test decreases with the next higher standard), in all scales except Terman. They are exceptionally numerous with the Simplex and Northumberland scales. To some extent, this might be due to an insufficiency of cases but such an explanation does not account for the fact that only some and not all the tests exhibit it. Inversions suggest poorly constructed tests. In each of the scales, Otis and National, one inversion occurs while the Terman gives one zero difference, that for St. VI to St. VII in Test 9.

In addition to inversions there are many places where tests fail to show reliable discriminative power, and this is true of all scales, Terman included.

The National Scale has the greatest proportion of tests which show discriminative power throughout the whole school range while Terman and Otis have each the same proportion of such tests.

A noticeable feature is the way in which the Northumberland tests, with the exception of A 4, fail to be of use with the top two classes.

Table VII. *Coefficients of Variability.*

Terman.

Standard	Test 1	Test 2	Test 3	Test 4	Test 5	Test 6	Test 7	Test 8	Test 9	Test 10
II—III	5·47	3·57	2·58	4·28	4·01	6·50	2·45	2·22	0·60	5·01
III—IV	6·86	5·78	2·50	5·71	4 47	2·78	6·23	2·51	6·19	3·85
IV—V	5·00	6·70	2·16	6·08	4·53	2·36	4·43	4·20	3·87	5·40
V—VI	5·85	7·80	3·67	8·12	11·60	5·10	6·88	3·59	5·57	4·94
VI—VII	4·16	1·64	5·31	2·17	6·08	0·63	1·49	0·88	*Zero Diff.*	3·42

Otis.

Standard	Test 1	Test 2	Test 3	Test 4	Test 5	Test 6	Test 7	Test 8	Test 9	Test 10
II—III	4·27	3·47	6·64	7·62	3·55	3·53	6·20	2·94	2·83	11·15
III—IV	4·18	7·71	4·66	1·63	8·57	6·43	6·59	4·90	6·53	5·39
IV—V	7·73	5 45	3·16	6·94	5·37	2·42	5·12	4·85	4·24	4·27
V—VI	2·47	5·00	3·95	5·45	10·80	5·88	7·64	1·67	6·91	5·98
VI—VII	2·10	4·49	3·99	4·82	2·35	0·78	3·20	*Neg. Diff.*	2·91	2·84

National.

Standard	Test 1	Test 2	Test 3	Test 4	Test 5
II—III	4·53	4·10	3·47	2·41	4·86
III—IV	8·08	12·57	5·71	7·54	5·13
IV—V	3·06	7·99	7·70	3·78	7·54
V—VI	3·16	3 80	4·98	5·81	3·89
VI—VII	9·17	4·08	*Neg. Diff.*	2·62	3·64

Northumberland.

Standard	A 1	A 2	A 3	A 4	A 5	A 6	B 1	B 2	B 3	B 4	B 5	B 6
III—IV	3·37	*Neg. Diff.*	4·62	5·85	3·62	1·94	6·04	1·14	5·22	8·23	5·93	0·80
IV—V	2·47	4·28	3·75	4·98	3·65	5·00	5·82	5·14	3·43	3·91	6·79	2·14
V—VI	0·26	4·02	3·15	2·62	6·18	1·93	3·23	5·63	4·55	5·90	4·77	3·90
VI—VII	*Neg. Diff.*	0·12	*Neg. Diff.*	8·73	1·29	2·79	0·88	2·12	1·60	*Neg. Diff.*	2·17	1·39

Table VII *a*. *Differences between averages for each test (Simplex).*

Group	A 1	A 2	A 3	A 4	A 5	A 6	A 7	A 8	A 9
IV—V	0·76	1·26	0·88	0·61	1·16	1·75	2·90	0·56	1·16
V—VI	*Neg. Diff.*	0·46	0·28	0·55	0·69	0·16	1·40	0·95	0·61
VI—VII	0·42	0·31	0·49	0·72	0·31	1·57	1·10	1·69	0·07

Group	A 10	B 1	B 2	B 3	B 4	B 5	B 6	B 7	B 8
IV—V	1·03	0·29	0·54	*Neg. Diff.*	0·54	0·96	*Neg. Diff.*	0·61	0·20
V—VI	0·41	0·16	0·82	0·71	0·14	1·12	1·49	*Neg. Diff.*	0·91
VI—VII	1·23	0·34	0·27	0·29	0·50	0·11	1·38	3·30	0·50

Group	B 9	B 10	C 1	C 3	C 4	C 7	C 8	C 9
IV—V	*Neg. Diff.*	0·15	0·47	0·70	*Neg. Diff.*	0·09	*Neg. Diff.*	0·27
V—VI	0·16	1·62	1·75	0·64	0·13	*Neg. Diff.*	*Neg. Diff.*	1·65
VI—VII	1·42	1·16	*Neg. Diff.*	*Neg. Diff.*	0·05	1·04	1·34	*Neg. Diff.*

The most discriminative of the Terman tests is No. 5 (Arithmetic). It shows particularly good discriminative power at all the levels we have chosen, not omitting the difficult one VI–VII. This test is similar to No. 1 in the National and to No. 5 in the Otis. The material is different and the number of items included varies, being twelve in the Terman, sixteen in the National and twenty in the Otis. The instructions for the Terman and the National are absolutely the same while the time limits are, Terman four minutes, National three and the Otis six. This test is undoubtedly useful for it ranks equally high among the tests of these other two scales, being first among those of the Otis and second among the National tests.

Test No. 1 ranks second in discriminative power among the Terman tests and like No. 5 is also capable of discriminating at all levels. In some respects it is superior to No. 5 because it shows slightly higher power at the lower levels. There is no similar test in other scales.

Examination of the Table will show that, excepting for the level VI–VII, No. 4 is of much value in helping the diagnosis of the scale. There are similar tests in both the National and the Simplex, varying only in the material used, in the time limits imposed and in the number of items included. Our tables show how these tests fared in the experiment. Remembering that this test is No. 3 in the National Scale, it is interesting to observe that with this as with the Terman it breaks down at the highest level while at all others it is remarkably valuable.

"Best Answer" apparently comes close to "Logical Selection" in adding its diagnosis to the score of the scale as a whole. They only break down at the one level.

A weaker test is No. 6 which resembles No. 3 in being irregular. There does not appear to be one like it in any of the other scales though both the National and the Simplex have "Word Meaning" tests. In the National it is termed "Same-Opposites" (No. 4). In the case of the Simplex (No. 8 A) it differs a little in form from these others in that the student has to select one word from five, not merely distinguish between two words. This naturally affects the marking. It is interesting to notice that in the Terman form the material discriminates badly up to St. V and after that very well for St. V to St. VI. This would indicate that the test was too hard for the juniors. It does not discriminate however between St. VI and St. VII. On the other hand in the National Scale the material suits the juniors better than the seniors. It would seem therefore, that with appropriate timing and material the value of this test could be greatly increased.

"Number Series" (No. 10) shows reliable discriminative values at all levels and is, in consequence, a valuable asset to the Terman Scale. It is also one of the best of the Northumberland tests, though there its range of suitability is small (Nos. A 2 and B 2). The same test occurs as 8 A in the Simplex.

The three remaining tests of this scale, Nos. 7, 8, 9, appear to rank roughly in this order. Nos. 7 and 8 are of about equal values at levels one and two and Nos. 7 and 9 at levels two and four. The Otis and the Simplex scales also have "Analogies" tests while "Mixed Sentences" occurs in both the Otis (No. 3) and the Northumberland (A 4, B 4), but "Classification" is particular to the Terman. The Otis form of "Analogies" discriminates remarkably well at all levels and must be considered as better assembled than the Terman. The Otis form of "Mixed Sentences" is distinctly better than the Terman and even better than either Northumberland A 4 or B 4, either of which is an improvement upon the Terman test.

We may conclude therefore that this scale is built up of tests which are in the main suitable for the range of the elementary school. While a few tests have comparatively weak discriminative powers with juniors others are notably good. A rather large number of tests fail to be of value at the upper limit, though there, Nos. 5 and 6 serve very well. The tests are moderately well assembled, show no inversions and but few irregularities.

The Otis scale has three tests capable of discriminating at each and all levels. These are tests Nos. 2, 3 and 7. Of these No. 7 is probably the best, *i.e.* as far as discriminating power is concerned, while No. 2 is somewhat better than No. 3. We have already compared Test 7 and Test 3 with the equivalent tests in the other scales.

No. 1 has distinctly high discriminative values at the lower ranges but at the higher is not so good. A test of this kind appears in the Simplex Scale and there, too (as far as the differences in the averages can show anything), seems to fail somewhat.

The "Proverbs" Test shows irregularity at the III–IV level, otherwise it is an excellent test.

No. 5 has at certain levels the highest discriminative power of any of the tests. It breaks down at the upper limit and examination of distributions of the scores indicates clearly that the test is too easy for these pupils. The equivalent tests in the Terman and National are particularly good at this level. It would therefore appear possible to improve the Otis form.

Test 6 is more irregular than any of the other tests. It is not so well assembled.

Test 8 is narrow in its range, narrower than any of the other tests in the Otis. The "Geometrical Figures" we found to be a great stumbling block to our pupils.

Test 9 aroused much interest but is, however, too linguistic for the juniors and altogether too easy for the seniors.

No. 10 is quite a valuable test, though of doubtful discriminative value at the high intellectual levels.

As a whole this scale is as well assembled as the Terman, even though there are irregularities, a few tests of narrow range and one inversion. Some of the tests are of great discriminative power at particular levels.

The National Scale has the greatest proportion of tests suitable for all ranges, with high discriminative values at each level. There are one inversion and no irregularities. Test 4 is the weakest of its constituents because of its narrow range. Test 3 though discriminating well at all other levels, seriously breaks down at the highest. This test together with No. 1 and No. 4 has already been compared with the equivalent tests in the Terman and Otis Scales.

Of the two remaining tests, the first resembles somewhat No. 9 of the Otis, though differing in that it does not supply the words from which selection is to be made. From the present point of view, the National appears to be the better form. The second is very valuable. It is a learning test. Though its originators felt its method of marking open to criticism, we experienced no difficulty with it. The test is a great asset to the scale.

The Northumberland Scale presents more inversions than any of the others hitherto considered. This shows that its usefulness is confined to very narrow limits. An outstanding feature of our figures is that section B is much more discriminative than A.

Test A 1 is too easy; its diagnosis value seems nil. B 1 a little harder test of the same kind is of much greater value.

Both A 2 and B 2 are very narrow in range; B 2 being, if anything, the better.

Tests A 3 and B 4 are very valuable members and again the B part is the better.

Nos. A 4 and B 5 are each valuable and, from this point of view, probably are the best units of the scale.

While Nos. A 5 and B 3 are of some discriminative value, the B test has a serious drawback in that there are two solutions possible for each of the items 2 and 5, while the scoring procedure and standardisation allow of but one.

The Hindustani test presented to the children a distinct novelty which riveted their attention. We too were struck by this novelty and were surprised to find the test of such little value from our present point of view. Apparently it is too hard in the B part and too easy in the A.

Summarising our results for these last two scales, we may say that the National is composed of well assembled tests of comparatively wide range showing no irregularities, but one inversion. It consists of only five tests. The Northumberland, on the other hand, consists of twelve tests, shows many inversions, few tests of wide range and some irregularities.

Little can be said about the Simplex Scale, beyond the fact that a very large number of inversions occur. Even though the numbers tested were small such inversions (when they do not occur for all tests), must point to poor assembling of the tests in which they come. We may also point out that the instructions are not clear as to whether children in seeking answers to the "Clock Face" test are allowed to hold their pages to the light and thus obtain an ordinary clock face to read. Many of the children saw this method of solution but we checked it. However on marking the papers we discovered that in many of the booklets the heavy printed clock faces came right through the paper and in these cases the candidate had no need to use any extra lighting. We feel that these influences must affect the usefulness of this test.

B. It is evident that a test should not give too many zero scores. Their presence in large numbers (1) alters the diagnosis of the scale, (2) affects its discriminative value, (3) tends to discourage the pupils and (4) adds a greater temptation to copy. We may say that a scale built of tests which give many zero scores is not of as much value as one which is made up of more flexible units.

Just how many zero scores there should be at the lowest level of intelligence the scale is framed to measure, is not decided. The authors in framing the National Scale sought to have no more than ten per cent. Examination of Table VIII will show that, of the tests we have used, few under this condition would be very useful for the lower standards of an elementary school.

The elements of the National Scale are, speaking generally, the most useful. With the single exception of the "Same-opposites" (No. 4) all the tests give few zero scores even with our Standard II children and consistently show smaller percentages as we ascend the school standards. Test No. 4, however, shows marked difficulty until St. IV is reached.

The other scales have only odd tests that show fewer than ten per cent. zero scores in all standards. There are, for example, two in the Otis—Nos. 5 and 10 (Arithmetic and Memory), but one in the Terman—No. 9 (Classification), while there is none in the Northumberland.

Table VIII. *Percentages of Zero Scores.*

		Standard					
Scale	Test	II	III	IV	V	VI	VII
Terman	1	62·5	24·3	7·1	1·4	0	0
	2	62·5	37·7	9·5	4·1	0	3·5
	3	62·5	54·0	38·0	33·0	22·8	10·7
	4	43·0	19·0	6·0	1·0	2·0	0
	5	75·0	37·0	25·0	15·0	2·0	0
	6	62·5	21·6	19·0	7·0	5·0	4·0
	7	25·0	5·4	2·0	1·0	0	0
	8	50·0	46·0	30·0	15·0	7·0	14·0
	9	6·0	0	0	0	0	0
	10	81·0	24·0	19·0	6·0	2·0	0
Otis	I	33·0	6·0	2·0	0	0	0
	II	0	14·0	0	1·0	0	0
	III	16·7	2·4	14·0	0	0	4·0
	IV	92·0	33·0	32·0	14·0	7·0	4·0
	V	8·3	0	0	0	0	0
	VI	33·0	22·0	2·0	4·0	2·0	0
	VII	25·0	8·0	2·0	4·0	2·0	0
	VIII	17·0	8·0	1·0	1·0	0	0
	IX	33·0	3·0	1·0	0	2·0	0
	X	0	0	0	0	0	0
National	I	7·0	2·0	0	1·0	0	0
	II	0	0	0	0	2·0	0
	III	0	5·0	4·0	0	0	0
	IV	21·0	28·0	6·0	3·0	0	0
	V	14·0	5·0	0	0	0	0
Northumberland	A 1	—	25·0	10·0	1·0	0	0
	A 2	—	50·0	55·0	33·0	20·0	20·0
	A 3	—	33·0	22·0	7·0	3·0	4·0
	A 4	—	75·0	50·0	25·0	16·0	14·0
	A 5	—	40·0	35·0	20·0	3·0	0
	A 6	—	50·0	31·0	20·0	12·0	0
	B 1	—	33·0	12·0	1·0	0·0	0
	B 2	—	60·0	56·0	31·0	25·0	14·0
	B 3	—	50·0	30·0	20·0	5·0	5·0
	B 4	—	35·0	11·0	6·0	4·0	4·0
	B 5	—	65·0	35·0	12·0	5·0	9·0
	B 6	—	50·0	37·0	20·0	5·0	0

We can indicate only a few of the interesting features presented by these tables. For instance, the percentage of zero scores is very much bigger in the Terman than in either of the other two American scales. It is noteworthy too, that equivalent tests in different scales, do not show the same percentages of zero scores. This is well marked in National No. 4 and Terman No. 3 and again in Terman No. 4 and National No. 3. Furthermore, some tests exhibit irregularities, having bigger percentages of zeros at higher standards than at low ones. Also the Northumberland

Scale shows very many high percentages of zero scores in the top standards. Test A 2 is a noticeable example. This mars its usefulness considerably. One other interesting feature of the Northumberland results is the smaller percentages of zero scores among the B tests than among the presumably less difficult A ones.

C. In the American Scales there were very few pupils who gained the full marks possible in a test. In fact in the Terman Scale, tests 2, 7 and 8 were alone fully completed by the elementary school children and these were only so completed by one pupil in each case. There were two tests in the National Scale which were simple enough for our pupils to gain full marks. Test No. 2 was completed successfully by two pupils, one in St. VI and one in St. VII. Test No. 5 was completed by two pupils both in St. VII. Five pupils gained the maximum mark in Otis No. 3, one in St. V, two in St. VI and two in St. VII. One St. VII pupil scored full marks in No. 5 and one St. VI pupil in No. 9 of the same scale. In the Northumberland the tests were all found simple enough for some pupils to gain the maximum score. Many of the tests gave maximum scores in the lowest standard and elsewhere the percentage of pupils who gained such scores was very high.

V. GENERAL COMPARISON OF THE SCALES.

The scales show correlation with both teachers' estimates of ability and examination marks. The correlations are rarely very high, but are higher with the American than the British. While Terman correlates slightly better with examination marks than the others, the National does so with the teachers' estimates. Probably the scale which correlates highest with these criteria (the only ones at our disposal) should be considered the best. Under these conditions, while there is but little difference between the scales we have used, Terman and National rank foremost.

These two scales occupy very favourable positions in our series, when viewed in the light of their discriminative powers, though here Otis is best. Our results indicate that the Simplex has high discriminative capacity but the numbers tested were insufficient to make direct comparison of its results with those of the other scales. The Northumberland results show that its range is somewhat limited. The other scales (Terman, Otis and National) are quite suitable for the whole range tested.

The individual tests comprising the scales vary considerably in their value. The National has the best balanced tests, while those of the Northumberland would appear to be worst. The results show that the scales admit of some improvement for in them are found erratic and badly balanced elements. As the results also show that the tests which are unsatisfactory in one scale are quite satisfactory in others, the way to improve scales is to some extent indicated. The percentage of zero scores in the various tests is least with the National Scale.

The American Scales are more difficult to administer than the British, but nevertheless take less time. Of all the scales the Simplex takes longest to give and longest to score. The American take about equal times, but of these the Otis is the most difficult to administer.

Any one scale, it is seen, may have a greater value than any other at an assigned level. For such particular application of scales, selection must be left an open matter. On the other hand, where the scales are for general use in elementary schools, the American scales are better than the British. Our order of preference for the former would be National A, Terman and then Otis. Our data would show that the published norms must not be considered as suitable for English pupils.

In conclusion, we must admit that many comparisons, and important ones at that, have necessarily been omitted here. The general question as to whether the scales do measure intelligence or not: the fairness of the weighting of the scores of the constituent tests: the rightness of the time allowances: the examination of the traits (and their number) presumably measured and other questions have purposely been avoided. It is not thereby assumed that they are unimportant. What we have done is to find to what extent each of the scales is capable of doing a particular kind of work—that is, assuming they measure intelligence, we have found the extent to which they and their constituent parts discriminate between pupils in elementary schools.

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SHOULD POEMS BE LEARNT BY SCHOOL-CHILDREN AS 'WHOLES' OR IN 'PARTS'

BY W. H. WINCH.

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I. THE PROBLEM STATED.

No modern aspect of English schools meets with more general support from educationists of every standpoint than that which emphasizes the value of poetry to school-children. If it is to be of the greatest value in later years, it must be known, and it should be known, as the author wrote it, and not with paraphrase or interpolation. Moreover, there is now no need to apologise for memory work; memory is admitted to be highly correlated with general intellectual ability, at least in the young, and is capable of improvement (within limits) by practice. Further, the newer developments in school method, such as 'individual work' and the 'Dalton plan,' throw much added emphasis on memorizing by the child. It is accepted then that poetry and much of it, if possible, shall be learnt. One of the problems involved is whether poems shall be learnt as 'wholes,' or 'in parts,' with a subsequent combination of the parts into wholes. That is the problem of the present paper. It is not intended to deny that 'interest' is an important factor, that a child will learn a poem it likes and understands better than one it does not; it is not denied that lessons in appreciation, given wisely, are helpful. On the contrary, these aspects of the case are, in my opinion, true and important, especially with adults. But when they are stressed, as they should be and have been in psychological work, there still remains the problem of the method of learning in such a way that the poem, if 'a thing of beauty,' should be 'a joy for ever.'

II. SOME RELEVANT PSYCHOLOGICAL CONSIDERATIONS.

It is well known that the span of apprehension and the immediate memory span grow in range and accuracy in school children as mental ability develops. Where the subject-matter is difficult, as is the case with nonsense syllables, Pechstein has shown that a 'part' method of memorizing is the better; and where the mental ability of the child is low, as is the case with 'mentally defective' children, Dr E. O. Lewis (in an unpublished paper) has also shown the 'part' method to be the better one.

Meanwhile, it has been asserted, not only for adults (on which matter I have no data) but also for school children, that the 'whole' method is the one that ought to be employed. In learning, let us say, verse by verse, though the child may be more accurate as to the verses themselves, it is maintained that there is much loss and difficulty in combining the verses. It is agreed that there is an amount of unnecessary learning in the 'whole' method, as a part well known may be unnecessarily repeated. A distinction is made between continuous poems and those in which the substance of each stanza is relatively distinct; the former is regarded as favourable to the 'whole' method, the latter rather more favourable to the 'part' method. A distinction is also made between immediate and final retention, and it is asserted that, if the 'part' method may be favourable in the case of immediate memory, it is certainly not so in the long run for retentive purposes. A further distinction is drawn between long and short poems, and it is thought that a relevant difference in method may be founded on that consideration alone.

Possibly I have sufficiently indicated the currents and cross-currents of opinion which are prevalent to-day, and I now turn to the experiments. I confine myself strictly to school children and to work done, as it must be done, if it is to confirm or negate a school method, under the usual conditions of school life.

III. THE FIRST EXPERIMENT.

i. *The children who did the work.*

The first experiment was performed with the whole of a class of 50 children (Standards V and VI *b*) in a municipal school for girls, rather well-situated in a fairly good suburban neighbourhood in the south-west of London. The work was conducted by the head mistress, who had already had years of experience in carrying out investigations in experimental pedagogy.

ii. *General plan of the experiment.*

Four poems were learnt by the girls, at proper intervals, and on the basis of the results of this work, they were divided into *two equal, parallel, and highly-correlated groups*. When this had been done, one group learnt six other poems, at proper intervals, by a 'part' method, and the other group learnt the same poems by the 'whole' method. Comparisons are finally instituted between the results obtained from the two methods.

Perhaps I need not now emphasize the need for obtaining equal mnemonic groups, if experiments of this kind are to give us a really clear and unambiguous result. Among other considerations, a significant value can be attached to rather small differences between the means of our groups if the series are highly correlated. For, even though the groups are initially equal, if they are not parallel and highly-correlated groups, the formula for the 'probable error' of those differences which must then be employed gives a high figure that often renders results which look conclusive on their face value, quite invalid. Huge and oscillating differences in any case are suspect: they are probably due to fatigue or lack of attention; the changes caused by the differences between any fairly-accredited pedagogical methods are not immediately great; but they are steady, and they are cumulative. In most cases, however, the children of high natural endowment will remain the best, by whichever method they work.

iii. *The method of learning the poems—the preliminary tests.*

Each girl was given a separate copy of the poem to be learnt and the following instructions:

> Learn as much of this piece of poetry as you can. You may learn it in any way you like without muttering. You will be allowed exactly ten minutes, and will then be required to write it out.

The girls in this school had been accustomed, as I have already indicated, to learn their poems verse by verse, and nearly all of them did so on this occasion: some, however, did not.

At the expiration of the ten minutes the girls wrote out all they could remember. One mark was allowed for every word correctly placed. If two words were correctly remembered, but transposed, one mark was given for the *two* words. No negative marks were given for omissions; any words, phrases or sentences following which were correctly remembered received appropriate marks.

iv. *The method of learning and marking the final tests.*

After the division into two equal groups, six poems were learnt, at appropriate intervals; but now one group learnt 'line by line,' and the other by a 'whole method.' The following were the instructions to Group A, which learnt by the part method.

> Learn a line at a time; when you know the first line go on to the second, and so on until you know the first verse: do not begin to learn the second verse until you are quite sure you know the first one: when you know the first verse, begin at the first line of the second verse, and learn that, then the second line, and so on until you know the whole verse; then see if you can say over to yourselves the first and second verses. Do not begin to learn the third verse until you think you know the first two. Then learn the third verse line by line, and go on like that until you have finished the poem.

I am not unmindful of the fact that the best units of apprehension, and probably therefore of memory, considered from the standpoint of meaning, are most probably not lines, but irregular segments depending on the thoughts involved. But I have not attempted to work out these units of apprehension for these poems; because, in the first place, the rhythm and rhyme of the lines—very important factors in enjoying and learning poetry—might thereby be affected, and secondly, my problem is a comparison of two methods which may be, and are, actually used by the ordinary teachers of schools under the usual school conditions. I do not wish to prejudge the question as to whether an initial marking off of 'units of apprehension,' some long, some short, by a competent psychologist, would not have produced mnemonic advantage. In the case of one of the poems given later, namely, *Dora*, this was actually done.

The instructions to Group B, which learnt by the 'whole' method were as follows:

> Read the poem over slowly and carefully from the first line to the last, trying to remember as you go along. Do not pause to read any line twice over before you go on to the next, but go straight on, trying hard to remember all the time. When you have reached the end, begin again at the beginning, and go through the whole poem again slowly and carefully in the same way. Keep on doing this until your time has expired.

The method of marking the poems when the girls had written all they could remember, was precisely the same as in the Preliminary Tests.

v. *Chronology of the experiment.*

Ten minutes were allowed for memorizing on each occasion, and an indefinite time for writing out what had been learnt. All the memorizing began at 9.55 a.m., following the usual Scripture lesson and a short lesson in Physical Exercises up to 9.50, five minutes being spent in giving out the necessary papers. All repetition, memory work and writing were avoided during the Scripture lessons, and all the usual recitation lessons, which in England mean the learning and reciting of passages in verse (or, perhaps, prose), were discontinued during the whole period of the experiment. The poems learnt were as follow:

Preliminary tests.

(1) *The Wind in a Frolic* ... Friday, June 26th, 1914, 93 words.
(2) *The First Swallow* ... Tuesday, June 30th, 1914, 94 words.
(3) *The Moss Rose* ... ... Tuesday, July 7th, 1914, 119 words.
(11 minutes were given in error for memorizing this poem.)
(4) *The Nightingale*... ... Thursday, July 9th, 1914, 115 words.

On the results of these preliminary tests the girls were divided into two equal, parallel and highly-correlated groups, regard also being paid to age; since, within the same class or grade, it is usually the younger children who are the more capable. The final tests were given as follows to both groups, to Group A which learnt by a 'part' method, and to Group B, which learnt by a 'whole' method.

Final tests.

(1) *Agnes*	Wednesday,	July	15th,	1914.	105 words.
(2) *The Arab to his Horse*	Thursday,	,,	16th	,,	124 ,,
(3) *Meg Merrilies* ...	Friday,	,,	17th	,,	118 ,,
(4) *The Evening Hour* ...	Tuesday,	,,	21st	,,	136 ,,

It will be noticed that these exercises followed one on the other rather rapidly, as we were making an endeavour to finish the work before the summer holiday. But as the poems hitherto learnt had been rather short, we did, in fact, continue the work after the holiday, and resumed it thus:

5 *a*.	*Dora* (part only)	Wednesday, Sept. 2nd, 1914.		452 words.
5 *b*.	,, ,,	Thursday,	,, 3rd,	,,
5 *c*.	,, ,,	Friday,	,, 4th ,,	,,

The girls wrote what they had learnt on the morning of September 4th, after the usual 10 minutes memorizing.

6 *a*.	*The Seagull* (part only)	Tuesday, Sept. 15th, 1914.		277 words.
6 *b*.	,, ,,	Wednesday, ,,	16th, ,,	,,
6 *c*.	,, ,,	Thursday, ,,	17th, ,,	,,

The girls wrote what they could remember on the morning of Friday, September 18th, at 10 o'clock, on which day the experimental work terminated.

vi. *Some notes from the observations of the mistress who conducted the experiment and marked the girls' papers.*

The usual method prescribed for learning poetry in this school is as follows: the teacher of the class reads through the 'poem,' with due attention to its elocutionary aspect: the girls then do the same collectively two or three times. Then the teacher gives a sort of summarized account of it, and explains any likely difficulties; then the poem is learnt by the girls, a few stanzas at a time; and, finally, from beginning to end. This procedure was *not* followed during this experiment; no preliminary reading occurred; no summary account and no explanations were given; the only instructions given were those that have been already indicated; the girls were now working entirely by themselves. The children, whilst memorizing, appeared during the first exercise to be working hard. Very few of them kept their lips still, though there was no audible muttering. (This feature may be the persistence of a primitive trait, useful possibly to very young children, but of doubtful utility to these girls, who averaged about 11½ years of age.) Some stared at the ceiling, saying over mentally what they had learned, some screwed up their eyes tighter and tighter as they drew near the parts more imperfectly known, one or two put their fingers in their ears, although there was no noise, either in the room or outside; they appeared to be trying to shut out all possible extraneous sounds. One girl finished learning the first poem in five minutes; she was satisfied she knew it; but, as a matter of fact, she only obtained 88 marks out of a possible 93. Two other girls stopped after eight minutes; they were also satisfied; but scored only 66 and 81 marks respectively. All the girls were told their marks after each exercise had been marked, and this tended to make the more confident girls give a longer time to subsequent poems. All of them had finished writing out what they could remember in 30 minutes; and, though there was no time limit, all but a few finished within 20 minutes. On the occasion of the second preliminary test the girls were steadier and seemed more earnest, if that were possible; they also appeared to think that the work was not so easy as they had thought the first time. At the fourth preliminary test it was noticed that they began and got into their full swing more quickly, and this characteristic became more marked as the tests proceeded.

vii. *Results.*

The experiment started with 50 children; but some children left the school during its progress, others were away ill, and some were absent on more than one occasion of the testing; their records and those of their 'paired' associates are, perforce, excluded. Thus the numbers available for final tabulation were reduced to 38. This is satisfactory for a continuous experiment lasting from June to September. The tabulated results follow:

Table I. *Showing the average results of Groups A and B compared, in the preliminary and final tests.*

Group A, working the final tests by the 'part method'

	Age on Sept. 30th 1914		Total marks for four prelim. tests	Total marks for four final tests	Marks for *Dora*	Marks for *Seagull*
	yrs.	mths.				
Averages of 19 cases	11	8	273	279	173	125

Group B, working the final tests by the 'whole method'

	Age on Sept. 30th 1914		Total marks for four prelim. tests	Total marks for four final tests	Marks for *Dora*	Marks for *Seagull*
	yrs.	mths.				
Averages of 19 cases	11	7	273	222	164	82

There is a differential advantage of 26 per cent. in the four final tests in favour of the part method: this differential advantage falls to 5½ per cent. in the case of *Dora* and rises to 52 per cent. for the *Seagull.*

The correlation between the *A* and *B* series in the four preliminary tests, worked out from the individual cases by the Pearson '*r*' formula, is + ·992: the difference between the means is negligible, its 'probable error' is negligible. In the four final tests taken before the school holidays, the correlation between the *A* and *B* series is + ·396: the difference between the means is 57, and its 'probable error,' calculated on the formula

$$\text{'p.e.'} = \cdot 67 \sqrt{\frac{\sigma_1^2 + \sigma_2^2 - 2r \, . \, \sigma_1 \sigma_2}{n}}.$$

is 8·7, which is about one-sixth of that difference.

The correlation between the two series of results for memorizing *Dora* is + ·27, the difference between the means is 8·0, and its 'probable error' is 11.

The difference between the means of the *Sea-gull* series is 43 and its p.e. is 8·0, the '*r*' coefficient being + ·313. The results may be shown further in the following way:

Table II. *Showing, section by section, the results of Groups A and B compared, in the preliminary and final tests.*

Group A ('part' method in final tests)

Marks in four prelim. tests	No. of girls	Av. marks in four prelim. tests	Av. marks in four final tests	Av. marks for *Dora*	Av. marks for *Seagull*
Over 300 ...	5	336	335	187	154
250 to 300 ...	8	275	273	167	125
Under 250 ...	6	218	241	172	101

Group B ('whole' method in final tests)

Marks in four prelim. tests	No. of girls	Av. marks in four prelim. tests	Av. marks in four final tests	Av. marks for *Dora*	Av. marks for *Seagull*
Over 300 ...	5	336	265	181	116
250 to 300 ...	8	274	224	160	73
Under 250 ...	6	218	184	158	64

viii. *Summarized conclusions from the first experiment.*

1. That school children of these ages and mental development learn poetry more accurately and rapidly by a 'part' than by a 'whole' method.

2. That this superiority of the 'part' method is much less clearly marked when the poetry learnt is continuous in meaning and without rhyme: the superiority in such a case is, indeed, statistically invalid.

3. That some discount from the generality of the first conclusion must probably be allowed for the fact that a 'verse by verse' method is the usually prescribed procedure of the school.

4. That there is evidence from many of the weaker papers that the usual preliminary reading, summary of meaning, and explanation of difficult phrases and words by the teacher prior to learning—the accepted

procedure of the school—is of considerable value in the subsequent memorizing; for their omission in the case of this experiment produced some rather unusual and confusing errors.

5. That the marking out into 'units of apprehension' instead of lines for the 'part' method in the case of *Dora*, though it may have been of value, did not prevent a closer approximation to the results of the 'whole' method than was the case with all the other poems consisting of several stanzas of less continuous meaning and possessing rhyme as well as rhythm.

IV. THE SECOND EXPERIMENT.

i. *The general plan.*

A second experiment was carried out from January to June of the year 1923. It was begun with the whole of Standards VII and VI with a few St. V boys forming the first and part of the second classes of a small municipal school for boys in the east end of London, attended exclusively by Jewish children. The district was a poor one; and, judging by the streets and houses in which the children lived, would be regarded as 'slummy.' But Jewish parents probably spend a smaller proportion of their total income on house-rent than Christians of the same social class; and their children, with few exceptions, have not the usual characteristics of slum children. These were well-fed, well-clothed, well-grown and intelligent boys, mentally in advance of Christian children of the same social class; partly, but not I think, wholly, on account of their earlier pubertic period. The school was not very strong pedagogically, though it had improved much under a new headmaster, who performed this experiment for me. The general plan of the research was as before. The boys doing the work were divided into *two equal, parallel, and highly-correlated* groups on the basis of the results of four preliminary tests in learning poetry. Then, after the two equal groups had been formed, six further exercises were given; one group learning by a 'part method'; and the other group memorizing the same exercises, but learning by a 'whole method.' There were one or two variations from the conditions of the previous experiment. The tests were more evenly spread out in time. Twenty minutes, instead of ten, were allowed for each period of memorizing, since most of these boys were a class or standard higher than the girls who did the previous work. The poems learnt were different and very modern, except in the 5th and 6th of the Final Tests, when we again used *Dora* and *The Sea-gull.* But now the whole of these two poems, instead of a part, had to be memorized. And finally, without any knowledge by the boys that it would be so used, and without further learning, *The Sea-gull* was required to be written out again three weeks after it was first written. *The Sea-gull* is rather a disjointed poem, and I wanted to see how far it had "fallen to pieces" after a considerable interval, and how far the method of learning, 'part' or 'whole,' had influenced the result. There is one other important difference between this and the first experiment. The usual method of learning poetry in the girls' school differed considerably from the one used in this boys' school. In this class the boys selected their own poems, each individually, from an *Anthology of Modern Verse*, and read them over by themselves until they knew them, then reporting to and being heard by the teacher of the class. I do not think the influence of a previous method would really overcome the natural advantages or disadvantages of the two methods we are testing, since we are working over a fairly long period; but it is well not to take risks and to try the experiment in schools where the official methods, so to speak, are different. The usual methods adopted in the girls' school previously cited might have been favourable to the 'part' method. If that is so, the usual method adopted by the teacher of the class now about to be cited is favourable to the 'whole' method. In both cases the method adopted during the course of the experiment differed from the ordinary procedure for learning poetry in the classes affected. That adopted for experimental purposes in the girls' school has been already described. In this boys' school there were also important variants from their usual method. The headmaster now took the boys, and prior to any conscious memorizing by the class, read the poem right through—he is a good elocutionist—outlined the 'argument' (if any) of the poem, and dealt with difficult words and phrases. The instructions to the boys, both preceding the preliminary and final tests, varied in no important particular from those given to the girls in the experiment previously described.

ii. *The chronology of the experiment.*

Preliminary tests.

1. *The Soldier* by Rupert Brooke. 115 words.	Wednesday,	January	31st	1923,	10.45 to 11.5 a.m.
2. *Sheep* by W. H. Davies. 136 words.	„	February	7th,	1923,	„
3. *Nod* by Walter de la Mare. 126 words.	„	„	14th	„	„
4. *The West Wind* by John Masefield. 264 words.	„	„	21st	„	„

Final tests.

1. *The Land of Story-Books* by R. L. Stevenson. 162 words.	„	March	7th,	1923,	10.45 to 11.5 a.m.
2. *The World* by W. B. Rands. 145 words.	„	„	14th	„	11.0 to 11.20 a.m.
3. *Baby's Song* by John Halsham. 135 words.	„	„	21st	„	10.45 to 11.5 a.m.
4. *Autumn in London* by Joan Druce. 127 words.	„	„	28th	„	„

(The Easter holidays here intervened.)

5*a*. *Dora* by Alfred Tennyson. 1446 words.	Wednesday,	April	18th	1923,	10.45 to 11.5 a.m.
5*b*. The same.	„	„	25th	„	„
5*c*. The same.	„	May	2nd	„	„

This poem was written out on May 3rd.

6*a*. *The Sea-gull* by Mary Howitt. 367 words.	„	„	9th	„	„
6*b*. The same.	„	„	16th	„	„
6*c*. The same	„	„	23rd	„	„

This poem was written out on Friday, May 25th, and again written, without relearning, on Friday, June 15th.

iii. *Notes made from observations by the headmaster who conducted the experiment and marked the boys' papers.*

The headmaster classifies the errors thus in order of frequency:

1. The omission of small words, conjunctions, prepositions and interjections.

2. Substitutions, such as 'on' for 'in,' 'and' for 'as,' 'out' for 'forth,' 'sits' for 'lies,' 'rides' for 'sails,' 'big' for 'tall,' 'boat' for 'ship,' with grammatical changes in the tenses of verbs and persons of pronouns.

3. Substance memory renderings, or as it is usually called in schools, 'paraphrasing,' *e.g.*, 'As he sits on the waves' for 'And the Sea-gull lies on the deep,' etc.

4. Omission of lines within the part known.

5. Omission of verses within the part known.

His impression was that every kind of error was more accentuated in Group A than in Group B.

iv. *Results.*

Table III. *Showing the average results of Groups A and B compared, in the preliminary and final tests.*

Group A, working the final tests by a 'whole' method

	Age yrs.	mths.	Total marks for four prelim. tests	Total marks for four final tests	Marks for *Dora*	Marks for *Seagull* 1st time	Marks for *Seagull* 2nd time
Averages of 26 cases	12	8	332	365	431	242	215

Group B, working the final tests by a 'part' method

	Age yrs.	mths.	Total marks for four prelim. tests	Total marks for four final tests	Marks for *Dora*	Marks for *Seagull* 1st time	Marks for *Seagull* 2nd time
Averages of 26 cases	12	9	331	412	405	260	238

The results may be shown further in the following table:

Table IV. *showing, section by section, the results of Groups A and B compared, in the preliminary and final tests.*

Group A, working by 'whole' method in final tests

Marks in four prelim. tests	No. of boys	Av. marks in four prelim. tests	Av. marks in four final tests	Av. marks for *Dora*	Av. marks for *Seagull* 1st time	Av. marks for *Seagull* 2nd time
		(To nearest units)				
Over 450 ...	4	499	496	922	363	361
350 to 450 ...	7	404	448	431	275	268
250 to 350 ...	8	298	349	376	215	180
Below 250 ...	7	203	235	211	158	118

Group B, working by 'part' method in final tests

Marks in four prelim. tests	No. of boys	Av. marks in four prelim. tests	Av. marks in four final tests	Av. marks for *Dora*	Av. marks for *Seagull* 1st time	Av. marks for *Seagull* 2nd time
		(To nearest units)				
Over 450 ...	4	489	535	615	329	329
350 to 450 ...	7	410	482	504	285	263
250 to 350 ...	9	290	383	360	260	188
Below 250	6	196	277	185	184	162

The '*r*' coefficient between the *A* and *B* series in the four preliminary tests is + ·988, the difference between the means is negligible, and its 'probable error' is negligible.

The '*r*' coefficient between the *A* and *B* series in the four final tests is + ·684: the difference between the means is 46, with a 'p.e.' of 12·7, a conclusive victory for the 'part' method.

For the *Dora* series the '*r*' coefficient is + ·265, the difference between the means in favour of the 'whole' method is 26, but its 'p.e.' is 44, so that no statistically conclusive victory can be claimed. A reference to the individual results shows that, in twelve cases of the paired associates, the 'whole' method gained the victory; and, in fourteen cases, the 'part' method.

For the *Seagull* series on the first occasion the '*r*' coefficient between the results for Groups A and B is + ·324: the difference between the means is 18 with a 'p.e.' of 14: not a conclusive victory for the 'part' method. Three weeks later the '*r*' coefficient is + ·388, the mean difference is 23 with a 'p.e.' of 13.

The '*r*' coefficient between the more immediate and the deferred reproductions of the *Seagull* by Group A is + ·829: the difference between the means is 27 with a 'p.e.' of 7·3. The corresponding figures for Group B are + ·936, 22, and 4.

v. *Summarized conclusions from the second experiment.*

1. Again, with the four shorter poems of the final tests, the 'part' method of learning has proved the better.
2. With the long, continuous poem of *Dora*, the balance of advantage is transferred to the group working by the 'whole' method: this is statistically invalid, but there seems to be evidence that some unusual factor in the nature of the poem itself, possibly its much greater length, or its continuity in style and meaning, or perhaps its coherence as a story and its absence of rhyme, or combinations of these factors, has rendered the 'part' method much less profitable than usual.
3. With the long, relatively disjointed and rhymed poem of *The Seagull*, the 'part' method has gained a victory, which becomes more certain after an interval of three weeks with no anticipation on the part of the boys that they would ever be required to write the poem out again. Even in these conditions the greater part of *The Seagull* has actually been remembered, and after three weeks' interval the boys learning by the 'part' method have retained a higher percentage of their original knowledge than those who worked by the 'whole' method.

The headmaster was astonished to find among the abler sections of the class how very little had become unavailable; in ten cases, five in each group, the boys improved on their previous records; and in five cases, two in one group and three in the other, exactly the same mark was scored.

V. THE THIRD EXPERIMENT.

i. *The general plan.*

The third experiment was also carried out in 1923, in the girls' department of the school described in the second experiment. The work was done at the instance of the headmistress, but was carried out by the teacher of the class, an able young assistant mistress. The experiment was performed with the whole of Standard VII; which, in this department, though not in the corresponding boys' department, formed a separate class. It was a homogeneous class mentally, though the ages of the girls ranged from 10 years 4 months to 14 years 3 months at the beginning of the work, and this homogeneity was clearly shown in the memory work of the present experiment. The general plan of the experiment was the same as of those previously described: the memorizing took place usually on Wednesdays after morning recreation (20 minutes being allowed for memorizing), one week later than in the boys' school. Both the preliminary and four final tests with short poems were the same, and given in the same order, as those used in the boys' department. We commenced with 26 girls; but four of them were absent from the preliminary tests on one or more occasions and their records are, perforce, omitted from the tabulation of the final tests. The usual method of the school in learning poetry was not the same as that obtaining in the boys' school. The teacher was accustomed to read to the girls the whole poem which was to be learnt; and then, verse by verse, to explain any serious difficulties of meaning or phrasing. Then the girls simultaneously read through the whole poem after the teacher, and then verse by verse, again with some note as to language. After this procedure the girls were told to learn the poem individually, verse by verse, and they were tested by the teacher, verse by verse, when they said they knew them; continuing in this way until the whole poem was mastered. The teacher of the class says that the general substance of the poem was learnt as a whole, and the actual words learnt verse by verse. In addition to these class exercises in memorizing poetry, each girl was expected to learn a short poem chosen by herself, one every month or so, and these poems were learnt by her in any way she chose. The work could not be carried beyond the Easter holiday, as it was in the boys' department; since so many of the girls were leaving that the balance of our groups would have been irretrievably lost. I am not able, therefore, to produce results with long poems as in the boys' school. The instructions given as to learning both the preliminary and final tests were exactly the same as in the boys' school: but whereas the headmaster gave a preliminary reading, outlined the substance of the poem and explained difficulties, in this case no preliminary work was done; the children began to learn each poem without any preceding reading or comment.

ii. *Observations from notes made by the mistress who conducted the experiment and marked the papers.*

As on previous occasions, one mark was given for each correct word correctly placed. In the case of a whole line or lines being omitted, full credit for the next line was given if the omission had been indicated in any way; if not, one mark was deducted: words inserted in the girls' papers which were not in the original poems were ignored, no negative marks being given. Of course, no marks were given for spelling, punctuation or handwriting. I asked the mistress to go very carefully through one set of marked papers in the final tests, and let me have her impressions as to the character of the comparative inaccuracies in Groups A and B. She chose *Baby's Song*, the third of the final tests, as it was the most difficult. In Group A, learning by the 'whole' method, only 6 girls out of 11 managed to remember the whole of it, even imperfectly. There are 10 long lines in all, containing 135 words, and of these 10 lines one of the girls reproduced 8, two others 6, and one of them only 4. In Group B, learning 'line by line,' only one girl failed to reproduce the whole, and she managed to rewrite 8 out of the 10 long lines. Moreover, the more trifling inaccuracies and transpositions were also more numerous in Group A than in Group B. The very serious difference in the results for this poem is accounted for by its difficult and non-continuous nature, for the other three poems of the final tests were fully reproduced, though, of course, not perfectly, by every child in both groups; except in one case, which has been dealt with in a footnote to the summarized table of results. Although the marks for Group A were lower than those for Group B, the errors in these easier poems were much the same in character for both groups.

iii. *Results.*

The summarized table of individual results follows:

Table V. *Showing the results of Groups A and B compared, in the preliminary and final tests.*

Group A, working the final tests by a 'whole' method					Group B, working the final tests by a 'part' method				
Name (initials only)	Age at beginning of the experiment yrs.	mths.	Total marks in four prelim. tests	Total marks in four final tests	Name (initials only)	Age at beginning of the experiment yrs.	mths.	Total marks in four prelim. tests	Total marks in four final tests
M.T.	10	6	535	547	I. B.	13	1	535	556
S. T.	13	6	511	551	M. S.	12	10	521	565
E. L.	14	2	504	547	F. B.	12	11	503	560
A. C.	14	3	498	452	D. L.	14	0	500	550
S. B.	10	4	493	542	R. F.	14	2	487	536
F. T.	13	8	484	488	R. S.	12	7	487	557
P. S.	13	0	483	*a.	M. B.	12	11	483	a.
N. L.	14	2	483	487	M. M.	13	11	482	550
K. F.	13	9	470	494	L. G.	13	7	481	547
J. O.	13	8	457	500	J. L.	13	2	459	493
G. F.	11	4	451	515	S. S.	11	4	447	555
S. R.	13	11	442	a.	L. R.	13	0	438	a.
L. S.	13	5	435	538	†L. P.	13	0	425	462
Averages ...	13	1	480	515	Averages...	13	1	480	539

* *a.* means absent on one or more occasions of the final tests. I have not omitted these names and marks and those of their paired associates in the preliminary tests, as the numbers are so small.

† Lily P.'s record is not satisfactory; she was unwell on the morning of the 2nd of the final tests; and though she wished to try and did so, she scored only 64 marks on this occasion as against 160, 116 and 122 in the first, third and fourth tests.

The correlation coefficient, worked out from the individual paired cases of the A and B series by the Pearson '*r*' formula on the totals of the four preliminary tests is + ·99, and the difference between the means is negligible, with a negligible 'probable error.' For the final tests the correlation coefficient is − ·063: the difference between the means is 24 with a 'probable error' of 9, which is less than ½ of that difference. There is a differential advantage in favour of Group B of 4·7 per cent. Omitting the unsatisfactory case of Lily P., and, of course, that of her paired associate, L. S., the '*r*' coefficient between the individual results of Groups A and B in the four final tests is + ·217; the difference between the means is 35 and its p.e. is 7, a very conclusive victory for the part method.

Again, with the four short poems of the final tests, the 'part' method has proved unmistakably the better.

VI. GENERAL CONCLUSION.

There seems cumulative evidence that the span of apprehension and immediate memory which is known to vary with mental development, is an important operative factor in memorizing poetry. Where the meaning and phrasing are difficult (as, to take an extreme case, with sequences of nonsense syllables) or where the grade of mental development is relatively low, the 'parts' apprehended and memorized at one span must be easy and short. Where the sequence is easy and follows in some close and clearly apprehended connection, the span of learning and memorizing may well be longer. There seems no doubt, that for school children of these grades and ages, a 'part' method of memorizing poetry of the usual form is decidedly superior to a 'whole' method.

(*Manuscript received* 5 *December* 1923.)

A FURTHER NOTE ON THE PHASE EFFECT IN THE LOCALISATION OF SOUND

By H. BANISTER.

(From the Cambridge Psychological Laboratory.)

In a previous note[1], on a new method which was being employed to investigate the phase effect in the localisation of sounds, I indicated that direct experimental evidence was being obtained against the Myers-Wilson[2] theory of the phase effect.

The Myers-Wilson theory states that, when there is a difference of phase between the sound waves arriving at the two ears, the intensity of the sound, at the ear at which the waves are leading in phase, is actually greater than at the other ear, although the amplitudes of the vibrations at the ear drums are equal. It explains how this intensity may be produced on the assumptions:

(1) that the sound waves are conducted from ear to ear through the bones of the head; and

(2) that the waves, arriving at the ear by bone conduction, differ in phase from the waves, arriving at that ear directly, by an amount $(\phi \pm \pi)$, where (ϕ) is the difference of phase produced by the difference in the paths which the two systems of waves, the direct and the bone-conducted, have traversed.

According to this theory, similar sound waves falling on the ears will give rise to a single sound, which will be localised towards the side at which the waves are leading in phase, because the resultant intensity is greater on that side. If the intensity of the sound at the other ear is now increased, it follows, from the theory, that the fused sound will gradually move over to that side.

A carefully controlled series of experiments has been carried out to test this deduction[3].

Observers were given sounds of the same pitch at the two ears, the sounds fusing to form a single sound, which was localised to one side. The intensity of the sound at the other ear was then gradually increased.

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[1] This *Journal*, XIII, 435 (1923).

[2] This *Journal*, II, 363 (1908); *Proc. Roy. Soc.*, A, LXXX, 260 (1908).

[3] Full details of the experiment will be published shortly.

There are three possible cases, depending on the initial conditions. These are,

(A), the sound waves 'in phase' at the two ears; and the sound localised to one side owing to greater intensity at that side;

(B), the sound waves differing in phase, the phase at the right ear, say, leading; but the intensities so arranged that only one sound is heard on the *left*;

(C), the sound waves differing in phase, the phase at the right ear, say, leading; and the sound localised on the right.

The resulting observations show that:

For case (A), out of 160 observations, one sound was reported 105 times, two sounds 38 times and the observer was doubtful 17 times.

For case (B), out of 91 observations, one sound was reported 66 times, two sounds 21 times and the observer was doubtful 4 times.

For case (C), out of 176 observations, one sound was reported 68 times, two sounds 82 times and the observer was doubtful 26 times.

Statistical treatment[1] shows that (A) and (B) may belong to the same 'population'; the difference between the two sets of observations is such as one would expect to find more than three times out of ten 'samples' taken at random. But neither (A) nor (B) has any connection whatever with (C); the variations between them are greater than would be expected once in 100,000 'samples.'

We are, therefore, driven to the conclusion, the reasons for which will be given in detail elsewhere, that the phase effect cannot be explained as an intensity effect due to bone conduction.

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[1] Mr G. Udny Yule, of St John's College, has very kindly looked through my working.

(*Manuscript received* 10 *May* 1924.)

NOTES ON THE PITCH OF A COMBINATION OF TONES

By PAUL R. FARNSWORTH.

(*From the Ohio State University, U.S.A.*)

Introduction (p. 82).
Theoretical considerations (pp. 82–83).
Experimental data (p. 83).
Training of subjects (pp. 84–85).
Conclusions (p. 85).

THE literature of musical aesthetics seems to show some difference of opinion concerning the pitch of a combination of tones. The generally accepted doctrine is that of Stumpf[1], who states that the pitch of a combination of tones is approximately that of the lower of its constituents. Valentine[2] takes exception to this, since his researches show "that the pitch of the higher note in any combination near the centre of the piano is likely to be an influential determinant of the apparent pitch of the combination."

In this connection, a test has been suggested by Stumpf. Subjects are asked which of the pairs, c° c′ or c′ c″, differs the more from c′ (c° presumably equals 129·33 d.v., c′ 258·65 d.v., and c″ 517·31 d.v.). Stumpf asserts that c° c′ differs more, thus apparently confirming his claim that c° gives the pitch of c° c′. Valentine tried this same test and found that nine subjects out of twelve asserted most emphatically that c′ c″ differs more, or in other words that the pitch of c° c′ is more like that of c′ than that of c′ c″.

Another test given by Valentine was to compare the pitch of g° g′ with that of b° d♯′. Eleven out of twelve subjects reported g° g′ higher, although g° is two tones lower than b°. This, again, apparently refutes the work of Stumpf. In this paper, an attempt will be made to reconcile the conflicting data with the aid of a few further experiments.

THEORETICAL CONSIDERATIONS.

In considering this matter, it seemed to me that there were three possible factors which might affect the responses of the subjects. Experimental conditions, therefore, were arranged whereby each was varied.

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[1] Stumpf, *Tonpsychologie*, 2te Aufl. xi, 384.

[2] Valentine, C. W., "The Aesthetic Appreciation of Musical Intervals Among School Children and Adults," *Brit. Jour. of Psych.*, vi, 1913–14, 190–216.

1. In most music, the upper notes of a composition carry the melody, and so we are trained to attend more strongly to these notes than to the lower. This means that the greater our musical training, the more apparent will the upper notes be to us. In the same way, an individual without musical training would not be expected to be influenced by this consideration at all.

2. In giving the order, "Which of these two tone combinations has the higher pitch?" the suggestive effect might enter. That is, with the above mentioned effect functioning, an individual who responds that g° g′ is higher than b° d♯′, since g′ is higher than d♯′, might be expected to report g° g′ lower than b° d♯′, since g° is lower than b°.

3. The last factor, which might be called the vocal base, is Stumpf's statement, which if true and not interfered with by the other three factors would assign the pitch to that of the lower constituent.

EXPERIMENTAL DATA.

Ten subjects, whose musical training was quite well known, were chosen and given the two Valentine tests. The octave below middle c and the two octaves above were used, and, as in Valentine's experiment, pianos were employed[1]. Stumpf's c° c′ c″ test was given to each subject three different times in the three octaves. On the second test eighty observations were made with each subject—forty with the question "Which is the higher?" and forty with "Which is the lower?" Twenty times g° g′ was given first with b° d♯′ second, and twenty times b° d♯′ was first with g° g′ second. To complicate matters, a° g′ was introduced in place of g° g′ forty times. The whole group was given in a random order in the three octaves previously mentioned.

In no case did the order of presentation make a difference greater than one response in ten (g° g′ and a° g′ or b° d♯′ first), so the results have been added together. It soon became evident that fairly strong differences in the hammering had no apparent effect upon the responses. In the case of five subjects, further observations were made with the instructions "How does the pitch of the first combination compare with that of the second?" This was done to eliminate factor two. The responses here were the same as with the usual instructions except that they were not as prompt. Additional verbal reports were not requested, but were volunteered in several instances.

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[1] As three octaves were used and the results added together, the ° here merely means that the note is an octave lower than one marked ′, and two octaves lower than one marked ″

TRAINING OF SUBJECTS.

Case A was a man, reared in a foreign country, with almost no musical training, not even that usually given in the grade schools. He reported three times out of three that the pitch of c° c′ differed more from that of c′ than did c′ c″. His 'higher' responses were 95 % for b° d♯′ when it was given with g° g′, but 50 % for a° g′ when it was given in place of g° g′. G° g′ was regarded as lower than b° d♯′ in 100 % of the cases, but a° g′ was considered lower in only 50 % of the observations. Case A was the only subject who responded to a° g′ in a manner different from g° g′. This might be accounted for by the fact that the poorly fused a° g′ made the elements stand out, and was therefore confusing. Case A, then, would agree nicely with Stumpf's findings.

Case B, also a man of no musical training, reacts quite similarly to A. For him, c° c′ was the more different from c′ three times out of three. His percentages were—'higher' b° d♯′ (with g° g′) 90 %, and b° d♯′ (with a° g′) 95 %; 'lower' g° g′ 90 % and a° g′ 90 %. Case B reported that the greater volume of the lower tone always made it more evident to him.

Since cases C, D, E, F, and G, show such similar characteristics, they can be considered together. C, D, and E were women, C having a good contralto voice. F and G were men, G singing a good bass. All had had special musical training. They all reported c′ c″ more unlike c′ than c° c′. In their 'higher' responses g° g′ and a° g′ were chosen from 95 to 100 % of the trials, and in their 'lower' responses b° d♯′ was chosen 100 %. Case C reported that the upper tone stood out for her because she had been trained to look for the melody in the upper series of notes. Most of the members of this group expressed surprise that anyone could judge the tones in a manner different from their judgments.

Cases H, I, and J were all individuals whose musical training had been limited to that ordinarily gained in the public schools. Case J did 'play at' a trombone somewhat, but had had very few lessons. In this group H and I were women. The responses here were all very variable. All were asked to consider the c° c′ c″ test four times and the votes were two for c° c′ and two for c′ c″ in each case. In the second test the results ranged from 35 to 60 %.

To put this in comparative form the following table is appended.

Question 1. Which pitch is more unlike c′—the pitch of c° c′, or c′ c″?

	Stumpf	Valentine	Farnsworth
No. for c° c′	All	3	2
Musical training	?	?	None
No. for c′ c″	—	9	5
Musical training	—	?	Excellent
No. tied votes	—	—	3
Musical training	—	—	Little

Question 2. Which has the higher pitch—g° g′, or b° d♯′?

	Stumpf	Valentine	Farnsworth
No. for b° d♯′	No results	1	2
Musical training	—	?	None
No. for g° g′	—	11	5
Musical training	—	?	Excellent
No. who did not vote consistently	—	—	3
Musical training	—	—	Little

CONCLUSIONS.

1. The ten subjects fall into three well defined groups:—(*a*) A and B; (*b*) C, D, E, F, and G; and (*c*) H, I, and J.

2. As has been stated above, even quite noticeable differences in intensities did not seem to alter the reactions.

3. Suggestion does not seem to influence the results to the slightest extent.

4. There seems to be no sex difference, since we find men in each group. I was unable to find a woman with as little musical training as cases A and B.

5. An almost perfect correlation seems to exist between extent of musical training and type of pitch judgment. Cases A and B, with no musical training, react in accordance with Stumpf's doctrine. Here the pitch of the lower constituent is of prime importance. H, I, and J, with some musical training, react sometimes in one way and sometimes in another. Cases C, D, E, F, and G, all trained in some form of musical endeavour, indicate that the apparent pitch of a combination of tones is for them the pitch of the higher constituent.

6. All this seems to indicate that an individual without musical training is influenced by the pitch of the lower constituent. This much agrees with Stumpf. It might be that an individual well trained in musical experimentation would also react in this manner. However, as a rule, as musical training is undertaken, the individual is taught to look to the upper series of tones for the melody. At length, the upper constituent becomes more influential than the lower. This is what apparently happened in Valentine's subjects.

(*Manuscript received* 10 *February* 1924.)

[NOTE.—Professor Valentine, in regard to the above paper, remarks: "My statement merely had reference to the majority of persons tested, who were good lovers of music, so far as I remember, but not experts."—ED.]

PUBLICATIONS RECENTLY RECEIVED

Primitive Ordeal and Modern Law. By H. GOITEIN. London: Grant Allen and Unwin, Ltd. 1923. pp. ix + 302. Price 10*s.* 6*d.* net.

This very interesting book is well worthy of the attention of the psychologist who is concerned with a study of social developments. In the first chapter there is a careful and well documented reconsideration of the question "What is law?" Then comes a study, partly historical and partly descriptive, of "the action at law." The author then leaves modern society and enters upon an investigation of various kinds of ordeal, chiefly as they have existed in somewhat primitive types of social organisation. Next comes a more detailed analysis of the Greek ordeal by water, and as the modern reader will expect, we now begin to see in legal procedure deep-seated evidence of regression towards a justice-preserving mother. Chapter five carries the study further and introduces definitely psychological, or psychoanalytical, notions. Law has been shown to be "something which has its roots deep down in human nature, whose origin is to be traced to the primal force which has shaped the destinies of the human race and whose development is intertwined with that of its social institutions." Mr Goitein, in his discussions on The Ordeal and Beyond, The Digging Stick, The Glebe Mother and the Coming of Law, tries to show some of the ways in which these deep roots have spread. He now returns to the Nature of Law, this time attempting a definitely psychological treatment, and concludes with discussions of Law, Custom, and Morality: of Rights, of Law in the Modern World, and finally of International Law. The book is one of great interest, clearly and pleasantly written. If it is not distinguished for outstanding originality of idea, it does nevertheless apply ideas which have already been more or less accepted to an important field of social activity. It would have gained in value had the author been a little more critical of some of his psychological authorities, but a discriminating reader can supply the criticism for himself, and meanwhile he will be grateful to Mr Goitein for a suggestive and stimulating study.

Aromatics and the Soul: A Study of Smells. By DAN MCKENZIE, M.D. London: William Heinemann (Medical Books), Ltd. 1923. pp. ix + 164. Price 7*s.* 6*d.* net.

It is to be hoped that Dr McKenzie will secure a large audience for this book, for he has produced a work delightful to read, full of sense as well as of fun, most pleasantly written. The chapters dealing with *The Sense of Olfaction in the Lower Animals, Olfactory Memory,* and *Smell in Folk Lore, Religion and History,* are particularly attractive. What the author calls his *pièce de resistance*, a study of *Theories of Olfaction*, is somewhat less satisfying, but it nevertheless contains a number of interesting suggestions. Of the book as a whole nothing but good need be said, and psychologists in particular, whether they are especially interested in smell or not, will find much to inform and delight them in its pages.

Osmics: the Science of Smell. By JOHN H. KENNETH, M.A. No. 2. Edinburgh: Oliver and Boyd. 1924. pp. 79. Price 2*s.*

The second number of *Osmics* contains bibliographical references to 500 papers or longer works on various problems of olfaction. There are also an admirable analytical index and an index of animals whose smell functions have been specially investigated. The book is well and clearly printed and is indispensable to anyone engaged in research upon the problems of smell. A brief indication of the contents of the more important publications would have added to the value of this very excellent work.

General Psychology. By Walter S. Hunter. Chicago: University of Chicago Press. 1923. pp. xv + 368. Revised Edition. Price $ 2.00.

This admirable text-book is, of course, already known in this country, but probably not as widely as it deserves to be. The revised edition is a welcome sign that there is still a general interest in the serious and careful presentation of psychology as a scientific discipline, apart from certain of its most recent and most sensational applications. Professor Hunter has a happy gift of lucid exposition, an excellent sense of the best order of arrangement of the subject-matter of psychology, and a wide knowledge of current methods and research. Withal his attitude remains somewhat conservative, and he does not dismiss as useless the work of the pioneers in experimental method. The new edition contains changes mainly in the discussion of Animal Behaviour, in the presentation of the mass of work lately carried out upon Individual and Applied Psychology, and in the section concerning images. Not perhaps remarkably original, the book is thorough, clear and interesting. It can safely be recommended to the serious beginner as a most efficient introduction to psychological study.

Mental Diseases: A Text Book of Psychiatry for Medical Students and Practitioners. By R. H. Cole, M.D., F.R.C.P. London: University of London Press. 1924. pp. xiv + 356. Price 15*s*. net.

This well-known text-book has now reached a third edition, and has been entirely reset and revised. The psychological portions of the book cannot be regarded as altogether satisfactory, particularly in that they hardly show as much knowledge of recent research as is desirable. Yet as a general introduction the book serves a very useful purpose and undoubtedly covers a large amount of ground. Sections on the legal bearings of mental disease are included. Numerous well-printed illustrations and plates add greatly to the value of the volume for the student.

Lola or The Thought and Speech of Animals. By Henry Kindermann, translated by Agnes Blake. London: Methuen. 1922. pp. viii + 188.

This is a book about a wonderful dog. It will be interesting chiefly to two types of reader: first to those who like remarkable tales of animal intelligence, and second to those who are interested in the relation between desired and actually obtained results in experimentation. Lola is exhibited as a kind of dog medium. But the investigations were insufficiently subjected to critical control.

An Outline of Psychology. By A. J. D. Lothian. London: G. Bell and Sons. 1923. pp. xi + 200. Price 5*s*. net.

This book is "an attempt to select those parts of psychological knowledge which are important or of special interest for educators." Within its prescribed limits the attempt is successful. The book is clear, very well arranged, and will serve as an introduction to modern methods of psychological study and their main results.

Principles of Psychology: the Foundation work of the Alétheian System of Philosophy. By Arthur Lynch. London: G. Bell and Sons, Ltd. 1923. pp. ix + 408. Price 21*s*. net.

In this book the attempt is made yet once again to analyse the complex operations of mental life into perfectly simple components, to show what these are, to define their nature with precision, and finally to display the structure and process of the most complicated mental activities by a recombination of those simple elements. The fundamental processes are said to be the following: Immediate Presentation, Conception of Unity, Association, Memory, Conception of Agreement, Generalisation, Impulse, Sense of Effort, Hedonic Sense, Negation, Conception of Time, Conception of Space. These are all discussed and their relations considered with great learning and at greater length. The discussion is often of very deep interest, and displays considerable insight, but the psychological reader will be bothered all the time to disentangle the psychology from the logic and epistemology. The fundamental processes do not all seem to be upon the same level of analysis, and when everything possible has been done by way of their combination, the complex processes of mental life will appear to many readers still to guard the secrets both of their structure and of the history of their formation. The claim made that "all modes of psychic acts" are explained by the system of this book is extravagant.

Mother and Son. By C. GASQUOINE HARTLEY. London: Eveleigh Nash and Grayson, Ltd. 1923. pp. viii + 318. Price 7*s*. 6*d*. net.

This is said to be a comprehensive interpretation, stage by stage, of the secret life which the child lives. It is influenced throughout by psychoanalytic theory, and so it deals far more with the latent processes and content of the child's life than with observations of outward behaviour. It is rather diffuse, and also it is apt to be sentimental. Much of the information which it gives is undoubtedly valuable, but it would have been more valuable still had the author made a serious attempt to show clearly what part of her general thesis is a matter of speculation and what part is undoubted fact.

Source Book in the Philosophy of Education. By W. H. KILPATRICK. New York: The Macmillan Company. 1923. pp. viii + 365.

This is an admirably collected and arranged set of quotations from most varied sources. The extracts are set out under twenty-two headings and deal with many problems of social psychology, social ethics, and education. For the teacher or student who wishes specific support from authority, this book will be extremely useful. It possesses of course the drawback of all such collations: it may induce readers to be satisfied with snippets.

The Development of Berkeley's Philosophy. By G. A. JOHNSTON. London: Macmillan and Co., Ltd. 1923. pp. vii + 400. Price 8*s*. 6*d*. net.

This is a lucid and admirable sketch of the growth of Berkeley's ways of thought. Their historical setting and their psychological basis are well shown. Probably the chapter on Berkeley's views on vision will be of greatest interest to the psychologist. The famous philosopher's theory is clearly expounded and sympathetically criticised, though the criticism does not display very much knowledge of recent experimental work.

Constructive Conscious Control of the Individual. By F. MATTHIAS ALEXANDER. London: Methuen and Co. 1924. pp. xxxiii + 317. Price 10*s*. 6*d*. net.

It is said that this book will "hit thinking people like a blow." The "blow," in this case, would be a very long drawn out one, for Mr Alexander has no lack of words, and no desire for economy in their use. However he says a number of very true things, and a good many important ones. He preaches "sensory appreciation"—a knowledge of environment and of psychophysical causation. To leave life to processes of automatic and unconscious adjustment is to court disaster. For environment changes more rapidly than adaptation, unless the latter can be consciously directed. No doubt this is true, and no doubt many of the exercises which Mr Alexander suggests are highly beneficial. As to whether his system meets all the claims he makes for it, however, is another matter. None but the expert could say.

The Measurement of Mental Traits in Normal and Epileptic School Children. By Dr J. E. W. WALLIN. Miami University Bulletin, Series XXI, No. 8. Price 75 cents.

This extremely interesting monograph records the results "of a comparative investigation of epileptic and normal school children by means of about a dozen independent psychological tests." Full information is given as to the tests used, and detailed tables of results are included. The general discussion is perhaps less complete than might be desired, but so far as it goes it is excellently and clearly set forth. All the epileptics tested were either backward or feeble minded, and both as regards initial performance and improvability showed themselves distinctly inferior to the normal children. Even in simple sensory tests this relation was maintained. Both as a record of work done and as an indication of the method of conducting comparative researches of this kind the monograph is to be recommended.

A Point Scale for Measuring Mental Ability. 1923 Revision. By ROBERT M. YERKES and JOSEPHINE CURTIS FOSTER. Baltimore: Warwick and York, Inc. 1923. pp. 219. $2.10.

In this revision of the first edition of the "Point Scale" monograph, two supplementary scales for testing intelligence have been added to the original pre-adolescent scale—namely, an infant scale and a scale for adolescents and adults. These two latter have not yet been adequately tested; but the pre-adolescent scale has been thoroughly tried out, and a set of norms for showing mental age compiled. The three scales are continuous, with many tests in common; and thus the mental functions are tested as they develop gradually from infancy to adolescence and maturity. In the Binet system, on the other hand, certain functions are arbitrarily selected to be tested each year. In the point scale, credit is given for each correct response, and not a mere all-or-none judgment of pass or fail for the individual question. Hardly any weighting of the tests is attempted, however; such weighting, if correlated with the results of experiment, might prove an advantage. But when more work has been done with it, the point scale system should prove a reliable method of mental testing.

A Critical Study of certain Measures of Mental Ability and School Performance. By INEZ MAY NETERER. Baltimore: Warwick and York, Inc. 1923. pp. 141. $1.90.

The object of the study described in this monograph was the investigation of the value of various intelligence tests, educational measurements, teachers' estimates of ability and marks of performance in prophesying school success and showing up individual differences; and their practical usefulness in selecting groups of children sufficiently uniform in mental ability to admit of efficient teaching. The results on the whole were negative. The inter-correlations of the various estimates of ability were not always significant and never high. Although classification of the children by chronological age, mental age, I.Q., or teachers' marks produced a more homogeneous grouping than that at present in existence, yet no one such classification provided the desired uniformity. This conclusion is of value in showing that homogeneous classes cannot always be obtained merely by grouping together children of the same mental age or I.Q

NOTES ON RECENT PERIODICALS

The Development of a Verbal Concept of Relationship in Early Childhood (*Scandinavian Scientific Review*, August 1923, pp. 32–83).

This is a contribution of unusual interest and originality. The experimenter and the subject sat on either side of a screen. The former gave a certain number of signals, 3 to 5, and the subject had to respond with similar signals in the same order. For example, the experimenter might ring twice with a bell and then make a tap with a pencil; hereupon the subject had to do the same with his own bell and pencil. Besides sound, similar experiments were made with pressure, colour, tactual form, and tactual roughness; in each case the stimuli were of two distinctly different kinds, these two being in random sequence. At the end of each sitting, the child's introspections were evoked by such questions as, "I wonder if you can tell me *how* you did it? How did you remember to give them back to me the right way?" The subjects were 56 children and were aged 5–7½ years.

As result, the introspections indicated that the mental processes fell into the following different classes:

(1) Apparently clear sensory memory, with no vocomotor or verbal imagery.

(2) Vocomotor counting (1 to 5, or 1–1–1–1–1), apparently without discriminating between the two stimuli in any conscious way.

(3) Summing up each kind of stimulus, but paying little or no attention to order of sequence (4 + 1, or 1 + 4).

(4) Vocomotor naming the successive stimuli (glass, glass, bell, glass, glass).

(5) Vocomotor counting (1–2, 1, 1–2).

(6) Vocomotor counting and adding at the same time (2 glass, 1 bell, 2 glass).

(7) Vocomotor naming, counting and adding at the same time.

Classes 1–4 are regarded as the first four steps in abstracting, and also in passing from generalization to particularization. 5–7 are further transitions to organized grouping of experience.

The general tendency was for the higher class to be adopted by the older child. But in respect of effectiveness, the matter was less simple; the second class did less well than the first; the third was only equal to the first; but from here onwards, the higher the class, the better the score.

Accordingly, "Our main outcome here seems to be, that an abstraction, away from the purely sensory impressions, is apparent with rising chronological age and with rising school-grade, but that this abstraction process may not necessarily mean any definite help as to actual amount remembered, as long as the child does not rise to a definite conscious plan."

The inter-correlations between the results for the different senses, as measured by the "foot-rule," average about ·40, whilst they correlate with the intellectual qualifications to an average of only about ·25. The sex differences are not marked.

The Journal of Abnormal Psychology and Social Psychology.

Vol. XVI, No. 1, April 1921.

EDITORIAL ANNOUNCEMENT. *The Field of Social Psychology and its Relation to Abnormal Psychology.*

With this volume, the Journal is extending its field to include that of social psychology, and the reasons for this step are here set out.

Personal Traits: Their Classification and Measurement. (Floyd H. Allport and Gordon W. Allport.)

Aims at presenting a working classification for the study of personality including the main divisions of Temperament, Self-expression or Strength, and Sociality. Traits were selected which were believed to stand for fundamental forces in human life. The rating of each subject with regard to the specified traits was made by three acquaintances independently, and compared with the answers to a questionnaire given by the subjects themselves. The resulting data are dealt with graphically, and four fairly distinct groups emerge, viz., the strong type of personality (extroverted) either social or a-social, and the weak type of personality (introverted) either social or a-social. An interesting minor point is the inverse relation occurring between intelligence and self-valuation.

The Standpoint of Social Psychology. (R. H. Gault.)

Conscious phenomena are held to be the first empirical data of the social psychologist, although the unconscious comes into his ken both in the form of instincts which precede conscious action and of acquired complexes which both precede it and arise from it. But there is no need to postulate the "social mind."

The Subnormal Mind versus the Abnormal. (Henry H. Goddard.)

A criticism of the assumptions involved in the use of the intellectual qualifications for the diagnosis of sub-normality and super-normality.

A Group Scale for Investigating the Emotions. (S. L. Pressey.)

An account of a "test form" which makes possible the inclusion of 600 diagnostic questions on a small blank which can be administered in thirty minutes and scored in less than three minutes.

Nos. 2 and 3. June–September 1921.

The Dilemma in the Conception of the Instinct. (W. E. Hocking.)

The author sums up his discussion in the following admittedly "hybrid" definition: An instinct is any specific form of the will-to-power which reaches its end by the use of innate motor mechanisms, common to the species.

The Role of Personality Development in the Reconstruction of the Delinquent. (Edith R. Spaulding.)

A general discussion of the social importance of the study of the development of personality, with a chart for recording traits of personality, and the analysis of two cases as illustration.

A Survey of the Physiology of Cerebration. (Geo. T. Johnson.)

A summary of the present condition of knowledge and opinion regarding the physiology of the cerebral processes, and attempt to explain "the development of experience" on this basis.

A Study of the Methods of Revivalists. (W. T. Shepherd.)

The results of a questionnaire addressed to ministers of religion and professional evangelists, with direct observations of additional evangelists.

The Psychoanalytical Method Applied to the Study of Repression. (J. H. Cooper.)

Repression serves as a gratification of a sexual desire towards the parent and represents a part of the sexual life of the individual, and resistance is an effort to retain the sexual pleasure derived from repressing.

The Fear of Action. (Pierre Janet.)

An address given to the American Psychopathological Society. A number of cases exhibiting an "anxiety of action" are described and Janet is "disposed to believe that in all these curious disturbances we are always concerned with mere depressions of activity, more or less profound, either involving the mind as a whole, or bearing down upon some one tendency or group of tendencies. The energy of performance being diminished but more especially the tensional level being lowered, the process of activation can no longer attain to the superior forms of behaviour."...The phobia of action is manifested "only when the subject is seeking to energize his performance under its higher form; thus, acting at a level of expenditure too costly for his budget of available resources."

Some Notes on the Stammering Problem. (Ralph Read.)

Remarks on the basis of psychoanalytic experience with stammerers. It is held that the problem admits of no general solution, but must be worked out for each individual.

The Mechanistic Features in the Dream Process. (Lydiard H. Horton.)

A "trial-and-error" interpretation of dreams, largely in terms of neural process, which offers the doctrine of "co-relativity," summarised as "the meaning of a dream-image is its relation to other co-ordinated systems of memory and is not absolute in itself, but gauged by the con-current operations of neurograms at the moment of dreaming."

Sleep and Dreams. (Knight Dunlap.)

A general discussion which, after summarising the physiological phenomena of sleep, and making some incidental remarks as to Freudian views, emphasises the need for further experimental research.

No. 4. October–November 1921.

Symposium on the Relative Roles in Psychopathology of the Ego, Herd, and Sex Instincts.

I. *The Ego Instinct.* (Bernard Glueck.)

Chiefly emphasises the difference between instinct as studied in animal behaviour and instinct as it operates in human life. Biology and physiology are of little value for the understanding of the ego-ideal; and it is here that the central problem of psychiatry lies, that is, in the nature and development of the "ego-complex," *its* relation to the component instincts of the personality, and to the environmental history. The author is inclined to emphasise the last factor.

II. *The Herd Instinct.* (Sanger Brown.)

Takes the view that in primitive societies "a collective kind of thinking.... indicated an ability on the part of the group to instinctively think and act as a group unit," and that this goes along with an undeveloped "sense of personality," since primitive man did not regard himself as intellectually entirely separate and distinct from other people, or quite distinct from inanimate nature.

III. *The Sex Instinct.* (C. MacFie Campbell.)

A brief note, mainly a protest against the assumption that all organic pleasure is sexual, and a plea for a less schematic and dogmatic formulation of the facts of human behaviour.

IV. *Synthetic View of Ego, Herd and Sex Instincts.* (John T. MacCurdy.)

Discusses the inter-relations of the three groups of instincts. The distinction is made between alternation of attention, and true instinctive conflict. It is held that the most important conflict is between sex and herd, and the second most important between ego and herd; the relation between sex and ego is rather one of alternation of attention than of conflict and repression.

The Role of the Risque Story. (Ralph C. Hamill.)

An analysis of a case in which it is shown that the risque story releases tension by turning the object of fear into a joke.

Nos. 5 and 6. December 1921 to March 1922.

The Use and Abuse of Instinct in Social Psychology. (William McDougall.)

An important critical survey of recent discussions of instinct in man. Dealing first with the critics of the concept of instinct in relation to human nature, the author analyses the difficulties inherent in the views both of those who deny instinct in man, and those who allow the concept, but narrow its meaning to discrete motor mechanisms.

The Value for Social Psychology of the Concept of Instinct. (Wesley Raymond Wells.)

Includes a discussion of the exact meaning of "inherited" and "acquired," only the *determiners* being actually transmitted.

A Hypoanalytic Study of Two Cases of War Neuroses. (W. S. Taylor.)

The term "hypoanalysis" is used as Hadfield and McDougall use it. The author gives the "explanation" to the patient, however, while the latter is still under hypnosis. "At that time he appears to be in such an objective dispassionate frame of mind, that he can check up or revise any hypothetical explanation which is submitted to him, provided he is requested to do so."

Expression of the Emotions in Man and Animals. (Wallace Craig.)

After stating the modern position of psychologists and biologists regarding the intrinsic importance, both to the agents and the patient, of expressive behaviour, the author goes on to a detailed criticism of Darwin's principle of "serviceable associated habits," and particularly of the view that "certain movements...are still pertinaceously performed through habit when *not of the least use.*"

The Quest for Objectivity in the Study of Human Phenomena. (Benjamin Ginzburg.)

Summarises recent contributions of French psychologists and sociologists, whom he considers to be more aware of the actual limitations of their science than British and American students are.

The Problem of the Individual. (Charles E. Cory.)

Reviews the problem in the light of the facts of dissociation of personality, and concludes that while it is in the sentiments that the unifying and stabilising functions lie, it is to the affective rather than the ideational element in the sentiments themselves that these owe their influence.

Psychometric Tests in Essential Epilepsy. (Winifred Richmond.)

The tests showed disorders of attention in every case, with poor motor co-ordination, poor vocabulary, an egoistic attitude, delayed reaction times and strong perseveration of responses.

American Journal of Psychology.

Vol. XXXIII, No. 1. January 1922.

Instinct and Value (H. C. Link.)

The author holds that both the mechanist and the behaviourist define instinct in such a way as to eliminate the concept of value, and, on the other hand, those who define instinct in terms of emotion and intelligence make it so easy to atrribute all values to instinct that the pursuit becomes everybody's game and nobody's science.

The Psychology of Reflex Action. (J. R. Kantor.)

A study of reflex action which leads the author to the conclusion that unless reflexes as well as every other type of reaction are considered definite psychological facts, rather than physiological acts, we cannot hope to understand them.

Functional Psychology and the Psychology of Act. II. (E. B. Titchener.)

This is a continuation of the article in Vol. XXXII, No. 4, and forms a survey of the two types of psychology in question. Both functional and "intentional" systems are held to be empirical, that is, technological; they begin and end with "mind in use." Functional psychology is "through and through teleological....Intentional psychology is at once more individual and less naive than functional....It is an applied logic, a logical account of psychical phenomena."

Affirmation and Negation. (C. H. Griffiths.)

An account of four groups of experiments, leading to the general conclusion that affirmation and the perception of identities are easier than negation and the perception of differences, and contradicting, as far as psychological processes are concerned, the view that all negation is affirmation.

Comparative Cognitive Reaction-Time with Lights of Different Spectral Character and at Different Intensities of Illumination. (Martha Elliott.)

Continuous spectra light produced a lag in cognitive reaction-time, as compared with the line spectrum light of mercury vapour; the light from incandescent tungsten produced a similar lag, as compared with normal white light; and the differences in reaction-time, in general, vary with intensity of illumination.

The Miracle Man of New Orleans. (John M. Fletcher.)

An Experimental Study of the Perception of Oiliness. (Lilian West Cobbey and Alice Helen Sullivan.)

Oiliness is a fusion of warmth and light pressure, which comes as a unique experience, being an intermediate sensory quality rather than a perception. The right combination of warmth and pressure yields the experience of oiliness, whether the actual stimulus be oil, water or a temperature cylinder.

Minor Studies from Cornell Psychological Laboratory include:

Cutaneous Localization and the "Attribute Order." (H. M. Lufkin.)

On the Non-Visual Perception of the Length of Vertically Whipped Rods. (Erna Shults.)

On the Non-Visual Perception of Horizontally Whipped Rods. (A. S. Baker.)

No. 2.

An Experimental Study of Children as Observers. (C. Comstock and H. Kittredge.)

A series of experiments which shows that given certain conditions children can be used as observers, and that a psychology of childhood can be built up experimentally.

Binaural Localization of Tones as Dependent upon Differences of Phase and Intensity. (H. M. Halverson.)

A study of binaural localization, first, as a function of the phase-relation of the tones at the two ears when the tone is conducted to the ears through closed tubes; second, as a function of phase-relation when the sources of sound are on either side of the head and the ears are open, with no conducting tubes; and third, as dependent upon the intensive differences of the tones at the two ears.

Mach's "Lectures on Psychophysics." (E. B. Titchener.)

A summary of Mach's lectures on Psychophysics, given at Vienna in 1863.

The Development of Meaning. (Raymond Holder Wheeler.)

An introspectional study, leading to the conclusion that the development of meaning involves three stages: (1) the original or "given" process; (2) a subsequent process which interprets the first; (3) a third process which constitutes a final interpretation. In every stage kinaesthesis is the core of the phenomenon which we call meaning, and is vital to the development of meaning.

Creative Imagination. (R. M. Simpson.)

An experimental study with a test involving "a visual imagery stimulus to creative action."

A Table for the Graphic Check of the Method of Constant Stimuli. (L. B. Hosington.)

Are There Any Sensations? (R. M. Ogden.)

A discussion of the problem in the light of (*a*) modern phenomenology and *Gestaltpsychologie*; (*b*) behaviourism; and (*c*) the newer views of neurology represented by Henry Head and his associates.

The Influence of Creative Desire upon the Argument for Immortality. (Cavendish Moxon.)

A study of Maeterlinck's argument for immortality, who, it is concluded, is "led to use bad reasons in support of his belief in immortality because this belief is a necessary symbol of his unconscious desires, which dominate the thoughts in his conscious creed."

A Bibliography of the Scientific Writings of Wilhelm Wundt. (E. B. Titchener and S. Feldman.)

Minor Studies from Cornell Psychological Laboratory include:

An Experimental Investigation of the Experience which Accompanies the Sudden Cessation of an Auditory Stimulus. (E. M. Amen.)

The After-Effect of Movement in the Sense of Touch. (W. A. Thalman.)

Haptical Illusions of Movement. (W. A. Andrews.)

No. 3. July 1922.

An Experimental Study of Certain Initial Phases of Abstraction. (H. B. English.)

A study of the process of abstraction, using visual and auditory stimuli. It is concluded that associative abstraction may suffice for the development of a general meaning, but the essential element in abstraction is a definite, though perhaps marginal, intention to consider a certain quality in isolation from any of its particular relations.

A Note on Wundt's Doctrine of Creative Synthesis. (E. B. Titchener.)

A study of the development of the doctrine in Wundt's thought, and of certain aspects of his personal "psychology."

Synaesthesia and Meaning. (R. H. Wheeler and T. D. Cutsforth.)

An introspective study of synaesthetic phenomena in a blind subject, with similar experiments upon an asynaesthetic blind subject, as a control. It is concluded that synaesthetic images "label" or "interpret an object," giving it a meaning; in other words, they are cognitive phenomena.

Series of Difference Tones Obtainable from Tunable Bars. (Paul Thomas Young.)

The Hydrogen Ion Concentration of the Mixed Saliva Considered as an Index of Fatigue and of Emotional Excitation, and Applied to a Study of the Metabolic Etiology of Stammering. (H. E. Starr.)

A research involving about 1300 salivary analyses and psychological diagnostic judgments. Stammerers are classified into four groups: (*a*) those, about 13 per cent., who are "sub-breathers" and have their organisms overloaded with carbon dioxide. For these, the remedy is a specific physical hygiene; (*b*) those, about 15 per cent., who are distinctly psychopathic and very hyper-excitable. These should be sent to a psychiatrist; (*c*) those who combine the conditions of (*a*) and (*b*); and (*d*) those who are simply hyper-excitable, without being psychopathic. The hydrogen ion concentration is considered to be a useful index of the condition of fatigue, and of the degree of emotional excitement.

Laughter, a Glory in Sanity. (Ransom Carpenter.)

A Note on Henning's Smell Series. (F. L. Dimmick.)

No. 4. October 1922.

Film, Surface, and Bulky Colors and their Intermediates. (Mabel F. Martin.)

An experimental study which indicates that the simple visual datum is a "film," predimensional, pretextural, non-localizable, and non-objective, with an attributive character of spatial spread or diffusion. There are no psychological intermediates between film and surface, but a sharp break, involving a complete change of attitude, between the film and the haziest of the surfaces, the immersed surface. The localisation of the color in the case of the surface is the determining factor. Between film and bulky color the shift of attitude is more tentative and cautious, but there are no true psychological intermediates. There is, however, a wealth of phenomenological intermediates, any one of which might have been stabilised as a perception if our everyday experience had required such a stabilization.

Can the Psychophysical Experiment Reconcile Introspectionists and Objectivists? (J. R. Kantor.)

Movements of Pursuit and Avoidance as Expressions of Simple Feeling. (Paul Thomas Young.)

Olfactory and auditory stimuli were used in this series of experiments. Critical analysis of the results shows that seeking movements indicate what might be called a need, irritant, incentive, or determination, rather than simple feeling, that "seeking" and "avoiding" are teleological interpretations of behaviour, and that the reputed correlations involve a confusion between meaning and affective processes. For these reasons it is recommended that the traditional hedonistic doctrine be abandoned and that psychologists study directly the facts of response and of affective psychology.

Diotic Tonal Volumes as a Function of Difference of Phase. (H. M. Halverson.)

Diotically perceived tones vary in their volumic aspect when the phase relationship between them is altered, and these volumic changes are of the same kind as those that accompany changes of pitch. Under the conditions of the experiment they were accompanied by changes of intensity.

An Experimental Study of Henning's System of Olfactory Qualities. (Malcolm K. Macdonald.)

On the basis of careful experiments, a detailed criticism of Henning's theory is offered, but it is concluded that the theory represents a first approximation to the truth. The smell prism does not, however, represent so definitely understood a system of qualities as does the color pyramid.

A Study of Liminal Sound Intensities and the Application of Weber's Law to Tones of Different Pitch. (Martha Guernsey.)

Weber's law as applied to audition apparently holds true with a fraction of about one-third throughout the middle range of intensities. The fraction is larger for low tones and for very high tones. It is also larger near the limen, decreasing universally in the third, fourth and fifth step.

Minor Studies from the Psychological Laboratory of Clark University include:

The Effect of Change of Intensity upon the Upper Limit of Hearing. (E. F. Moller.)

A Study of the Relation of Distracted Motor Performance to Performance in an Intelligence Test. (Miles A. Tinker.)

Minor Studies from the Psychological Laboratory of Cornell University include:

The Areal and Punctiform Integration of Warmth and Pressure. (Ida Bershansky.)

The Integration of Warmth and Pain. (Lucille Knight.)

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

May 17, 1924. On the Influence of Unlearned Material upon Tasks performed with Learned Material, by Rev. R. C. McCarthy.

The Causes of the Continuous Change of Resistance observed in Psychogalvanic Experiments, by R. H. Thouless.

SECTIONAL MEETINGS.

(a) Education Section.

April 14, 1924. An Experimental Study of Mathematical Ability of Boys and Girls in Secondary Schools, by Miss A. E. Cameron.

May 26, 1924. Should Poems be learned by School Children in "Wholes" or in "Parts," by W. H. Winch.

June 16, 1924. Some Modifications in the Teaching of the Three R's to Mental Defectives, by D. Kennedy Fraser.

(b) Industrial Section.

May 28, 1924. Departmental Co-ordination and its Influence on the Worker, by A. Angles.

(c) Medical Section.

April 30, 1924. The Significance of the Mouth in Psycho-Analysis, by Dr Edward Glover.

June 25, 1924. The Pathology of Tremor in relation to the Neurosis, by Dr R. G. Gordon.

VOLUME XV OCTOBER, 1924 PART 2

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EIDETIC IMAGERY

BY GORDON W. ALLPORT.
(*From the Cambridge Psychological Laboratory.*)

I. *The Phenomenon* (pp. 100–101).
II. *Descriptive and Functional Characteristics* (pp. 101–113).
  1. *Localization* (pp. 102–103).
  2. *Richness in Detail* (*Clearness*) (pp. 103–104)
  3. *Persistence* (pp. 104–105).
  4. *Intensity* (*Weight*) (pp. 105–106).
  5. *Corporeity* (p. 106).
  6. *Size of Visual Field* (p. 106).
  7. *Coloration* (pp. 106–107).
  8. *Conditions of Arousal and Disappearance* (pp. 107–108).
  9. *Selective Tendencies* (pp. 108–109).
  10. *Flexibility* (pp. 109–110).
  11. *Coherence* (pp. 110–111).
  12. *Plasticity* (p. 111).
  13. *Invariability* (pp. 111–112).
  14. *Displacement in Space* (p. 112).
  15. *Fusion* (pp. 112–113).
III. '*Memory-levels*' *and* '*Undifferentiated Unity*' (pp. 113–115).
IV. *Theoretical Note* (pp. 115–119).
V. *Summary* (pp. 119–120).

THIS paper is concerned with an examination of the researches into visual memory imagery which, during the past five years, have issued from the Marburg Institute for Psychology, under the direction of E. R. Jaensch. These researches claim to investigate certain unique and hitherto unrecognized characteristics of imagery in children. The work is of immediate importance, both to practical pedagogy and to theoretical psychology. Its consequences are far reaching; indeed, in the first flush of discovery the Marburg school has based a comprehensive doctrine of the evolution of mental life upon the phenomenon which it has brought to light. Before discussing this doctrine, or offering any interpretation of the phenomenon, it will be well to enquire into the nature of the problem investigated, and to examine critically the evidence upon which the Marburg school bases its theory. From time to time during the discussion I shall have occasion to draw on the records of my own investigations among eleven-year old children in the schools of Cambridge.

I. *The Phenomenon.*

Urbantschitsch, who seems to have been the first to consider eidetic imagery as a phenomenon of unique significance, states the case as follows: "Among optical memory-images we find in addition to the customary 'visual image' an *Eidetic Image*[1]. In the one case a former visual perception is merely 'imagined,' in the other case the original object is actually 'seen.' A person may be able to remember clearly a visual experience and to describe it in detail without necessarily possessing eidetic images. The true eidetic image, in distinction to the visual memory-image, revives the earlier optical impression when the eyes are closed, in a dark room, and sometimes when the eyes are normally open, with hallucinatory clearness[2]." In order to delimit more accurately the field of investigation, this definition should be understood to exclude both pathological hallucinations and dream images, and to admit those spontaneous images of phantasy which, though possessed of perceptual character, cannot be said to be literally revivals or restorations of any specific previous perception ((13), p. 1).

This special type of psychical after-image is probably in essence the same phenomenon that has at various times been mentioned under different names, such as *subjective vision*[3], *memory after-image*[4], *primary memory-image*[5], *projected memory-image*[6], and *imaginary perception*[7]. Earlier observations, however, were confined to adults, and the true significance of the phenomenon was overlooked. To the Marburg school belongs the credit of first exploring the subject systematically and of referring it to its proper sphere—child psychology.

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[1] Urbantschitsch employed the term *Anschauungsbild* which has been adopted by all subsequent writers. The capacity for imagery of this type is designated by Jaensch and his school as the *eidetische Anlage*, and the individuals who possess this capacity as *Eidetiker*. It seems best to translate *Anschauungsbild* as 'eidetic image,' in order not to complicate unnecessarily the terminology, though 'eidographic image' would perhaps suggest better the true nature of the phenomenon. Urbantschitsch published his work in 1907 under the title *Über subjektive optische Anschauungsbilder* (Leipzig, Vienna).

[2] Cited by Kroh ((13), p. 1). References in bibliography.

[3] Müller, *Phantastische Gesichtserscheinungen*, 1826.

[4] Fechner, *Elemente der Psychophysik*, II, ch. XLIV (*b*).

[5] Ward, *Psychological Principles*, pp. 175 ff.

[6] Martin, "Die Projektionsmethode und die Lokalization visueller und anderer Vorstellungsbilder," *Ztsch. f. Psychol.* LXI, 321–542, 1912.

[7] Binet, *Psychology of Reasoning*, Transl. Second Edit., pp. 136 ff., 1899.

The studies which have been made of the extent of eidetic powers show that approximately 60 per cent. of all children between the ages of ten and fifteen are able to produce eidetic images ((13), pp. 18, 47; (7), p. 7)[1]. Statistics for younger children are as yet lacking. In different types of schools and among different races the proportion of *Eidetiker* varies (unpublished report of Jaensch before the Eighth Congress for Experimental Psychology at Leipzig, 1923). There is an observable retreat of the ability during adolescence ((13), p. 47; (7), p. 8), though among poets and artists a large number, perhaps the majority, are in respect to their imagery 'grown-up children' ((14), pp. 118 ff.).

The technique for the investigation of the eidetic phenomenon need not be elaborate. In my own investigations I have used a simple projection mat (24 inches square), covered with dark grey paper, resting on a table at normal reading distance from the observer. The edge of the mat farthest from the observer was raised so that the screen made an angle of 30° with the table. Upon this mat I placed, one at a time, pictures cut from an ordinary child's picture book. The pictures were rich in detail, the principal features being in silhouette, and objects in the background coloured in delicate tints. The time of presentation was 35 seconds. The child was instructed simply to observe the picture carefully and to report what he saw upon the mat after the picture was taken away. After such instruction the child with well-marked eidetic powers usually begins immediately with an account of the central features of his image; questions may sometimes be necessary to bring out the finer detail. Such questions, needless to say, must not be leading, and it is usually sufficient to ask the child to describe certain features 'more closely.' Children with less eidetic ability frequently need to be instructed regarding the nature of the phenomenon; but in such cases the investigator must determine by careful cross-questioning whether, or to what extent, the child's report is merely the effect of suggestion. Other points regarding the technique of investigation will be considered in the following section.

II. *Descriptive and Functional Characteristics.*

In examining the psychological nature of the eidetic image it will be found profitable to make each characteristic serve, so far as possible, as a criterion for distinguishing the eidetic image (EI) from the visual memory-image (MI)[1] and the after-image (AI).

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[1] My own records taken from eleven-year old children in Cambridge schools show that only about 50 per cent. are able to produce what may unequivocally be designated as eidetic images; Kroh finds the proportion for the same age-level in German schools to be 65 per cent.

1. *Localization.* All investigators agree in ascribing to the EI a certain 'perceptual' character. That is to say, the content of the EI is "in a literal and true sense 'seen'" ((7), p. 3); the attention of the observer is directed *outwards* ((13), p. 24); and there is a noticeable tension and pressure felt in the visual organ "incomparably stronger than that which attends the MI" and often "stronger than that which attends ordinary accommodation for near objects" ((13), p. 28). This attribute of external localization affords an unmistakable distinction between the MI and the EI, but does not in itself serve to distinguish adequately between the EI and the AI.

When the eyes are open the EI appears in most cases against some smooth ground, such as the wall of the room, a blackboard, or (most favourably) a grey mat. "The majority of all observers see the EI in the plane of the background, a small minority localize it between the ground and the eye. It never happens that the image is entirely unlocalized.... With many of the observers the EI is so closely bound up with the ground that it distorts itself with the ground when the latter is folded or bent" ((10), pp. 281 f.). Not all investigators agree in this matter. Thus Gösser speaks of the usual "confused and cloudy" appearance of the ground, from which the EI tends to 'free' itself ((4), p. 104). Several investigators incline to the view that the character of the localization is dependent upon the attitude of the observer ((4), p. 126; (2), pp. 325 ff.; (18), p. 60). The best evidence seems to point to an identical behaviour of the EI and the AI in this respect: "An attitude of one sort may give the AI sharp outline and definite texture...but as soon as this 'objective' attitude becomes ever so slightly critical, the localization ceases to be definite, the outlines are blurred, and the image is again essentially soft and insubstantial[2]." This finding is in agreement with that of Katz, who states that of itself the AI is textureless, dimensionless, and without locality (*flächenfarbig*), but that it readily adopts the spatial character of the ground under certain conditions of fixation[3]. It is reasonable to suppose that children, as well as untrained adult *Eidetiker*, with their usual concrete (*dinghaft*) attitude, are, in the majority of cases, unable to assume the 'critical' attitude; and hence the present evidence regarding the nature of the localization of the EI in perceptual space must be regarded as inconclusive.

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[1] To be understood as equivalent to the *Vorstellungsbild*, or purely central 'idea image.' In a certain sense, of course, the EI and the AI are likewise memory-images (*Gedachtnisbilder*).

[2] Braddock, C. C., "An Experimental Study of the Visual Negative After-Image," *Amer. Journal*, xxxv, April 1924, p. 166.

[3] Katz, D. *Die Erscheinungsweise der Farben*, pp. 56 ff., Leipzig, 1911.

In cases where the observer is in a dark room or where the eyes are closed the EI still appears to be projected; it is never localized 'within the head.' A judgment may readily be secured from the observer concerning the relation of the image to the retinal grey which, when the eyes are closed, becomes a kind of 'visual space.' In many cases the EI appears to be 'etched' into the grey and in others to be 'surrounded' by it ((10), p. 283). Kroh asserts that preceding the arousal of such EI there is always a brightening of the visual field, a phenomenon which he considers to be identical with that of retinal light ((13), pp. 25, 44).

Finally it should be noted that the EI, though possessed of an 'outer' character, like a true perception, is almost always recognized as a distinctly *subjective* phenomenon. This recognition may be accounted for in part by the voluntary effort which is usually required to produce the image ((13), p. 70), and in part by the fact that the EI is easily displaced from its position in phenomenal space by eye movements (in which case the EI is always in the centre of visual attention) ((2), p. 323; (10), p. 54; (7), p. 25).

2. *Richness in Detail* (*Clearness*). In cases where the relative richness of detail has been studied in all three types of images, it has been found that, excepting where the eidetic ability is very rudimentary indeed, the EI far surpasses both the MI and the AI. The superiority over the AI is accounted for by the fact that 'complicated' objects (*e.g.* pictures) are well suited to the production of EI whereas they generally fail entirely to arouse AI ((1), p. 10). On the other hand the MI of even the best ordinary visualizers is poor in detail compared with the images of a true *Eidetiker* ((13), p. 29).

When asked to give an account of a picture 'from memory' my observers have all been able to describe the content of their MI without having recourse to the EI; and then upon direction to turn their eyes to the projection mat and to supplement their account with what they 'see' there. With scarcely an exception the EI supplies detail which in the MI was lacking. It sometimes happens that a child upon the evidence of his EI corrects spontaneously misstatements which he made in his previous account 'from memory.' It is more usual, however, to find that a false MI results in a false EI. Among most of my observers, in fact, the report of detail from the EI was more accurate when no MI was previously called for (cf. discussion of the criterion of Plasticity). Though

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prejudiced by a preceding MI in regard to accuracy, the EI (of a true *Eidetiker*) is always superior in *amount* of detail.

This characteristic of clearness affords perhaps the best ground for distinguishing the EI from the MI. The clearness of an MI is more dependent upon a definite 'structuration' in its content. "When some detail of a presented object is unusually outstanding, the other features are readily grouped around this detail, and the whole image tends to acquire a distinct and well-defined character.... Every observer brings to his task of interpreting and responding to a presented situation more or less well-marked tendencies to behaviour and to affectivity.... Certain details in presented situations excite these tendencies more readily than do others. These constitute, probably, the most numerous class of details which are designated 'striking,' and it is upon them that the formation of clear and distinct images (MI) in the main depends[1]." The case of the EI is somewhat different. There are to be sure 'selective tendencies' (*vide infra*, p. 108) which condition the arousal of the image according to the interest of its content for the observer; but once the image is aroused the representation of the original object is mimetic rather than selective.

It is not to be supposed that the representation is strictly photographic in accuracy. The EI, as will be shown, frequently undergoes extensive and remarkable alterations from the original. It is furthermore often very incomplete in character. Many children seem to have only the rudiments of the required ability; they secure simply outlines or partial detail, and in such cases the EI is obviously *less* clear than the MI. But, with all its flexibility and occasional incompleteness, the EI for the gifted child has a capacity for mirroring detail in a sense far more literal than is the case in the richest of MI. The precise number of buttons on a pedestrian's jacket, the letters composing a word in a *foreign* language on a poster in the background, the length and direction of the lines of shading in a stretch of roadway, the number of whiskers on a cat's lip—such are the amazing details, fundamentally unrelated to any 'nucleus' in the original presentation, which the true *Eidetiker* is able without effort to report[2].

3. *Persistence.* The continuance of the EI in the visual field even through protracted periods of observation and report serves as an important criterion for distinguishing the EI from the AI. Distraction of

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[1] Bartlett, F. C., "The Functions of Images," this *Journal*, XI, April 1921, p. 325.

[2] Kroh found that among 37 eleven-year old *Eidetiker*, 23 (or 62 per cent.) have these remarkably clear EI ((13), p. 18). I have found among 30 *Eidetiker* of the same age-level only 3 cases (or 10 per cent.). It may be that this discrepancy is due to racial differences, but the number of cases is too slight to support such a generalization.

attention usually causes at least a temporary disappearance of the EI ((13), p. 30; (1), p. 6), but on the whole the EI is far more stable than the AI. Little evidence seems to be available regarding the maximal duration of the EI. Herwig cites cases where the eidetic ability in general is weak and where the images are of short duration ((5), p. 140); but for the true *Eidetiker* the image presumably may endure so long as it affords an object of 'absorbing interest' to the observer ((13), p. 30).

Important in this connection is the phenomenon of *recurrence*. By voluntary effort the observer "is able after hours, days, and even months and years to reproduce an EI with all its previous vividness" ((13), p. 30). This is a contention which sorely needs testing. As yet there are but a few case records where reproduction after an interval of time has been studied in detail (see, *e.g.* the stenographic report of the account given by a fourteen-year old *Eidetiker* taken five weeks after the original presentation: (7), pp. 36 ff.). That the EI is subject to voluntary recall after the lapse of considerable time affords an infallible criterion for distinguishing it from the ordinary AI. Cases are reported where the EI takes an obsessive character and recurs without volition, *e.g.* in day dreams and in characteristic childish fears ((13), p. 67). Such EI are not confined to reproductions of previous perceptions, but often are simply 'imaginary perceptions' (spontaneous EI), and in extreme cases approach the category of true hallucinations ((13), pp. 101, 189).

4. *Intensity* (*Weight*). When images, whether MI, EI or AI, are projected against a non-homogeneous ground a certain rivalry takes place between the image and the ground. Busse finds that the details of the ground diminish in clearness to varying degrees for each of the three types of images, with a corresponding increase in the intensity or weight of the image: $AI > EI > MI$. That is to say, the EI in comparison to its ground is less dominant than the AI, but more dominant than the MI. In certain cases, however, the details of the ground for all types of images become completely suppressed so that nothing but the content of the image is visible; or, on the other hand, the ground sometimes dominates in such a way as to cause an almost total disappearance of the image. In such extreme cases the intensity for all images is equal: $AI = EI = MI$ ((1), pp. 27 ff.).

Busse's results are rendered doubtful, however, by the fact that she exposed the object which was to arouse the image for varying lengths of time: 5 seconds for arousal of MI, 15 seconds for EI, and 40 seconds for AI. The longer exposure might well be the cause of the superior intensity of the AI (cf. (11), p. 130; (18), p. 79). It is worth noting likewise that Busse always worked with the different images in the same order, viz. MI, EI, AI, and required the observer to give his judgment only after the series of three was completed. It would be impossible from this method to rule out the time error, the more so since all of her observers were children (average age 13½).

5. *Corporeity*. Busse's conclusion that to an *Eidetiker* the MI appears as two-dimensional, the EI in semi-relief, and the AI as completely three-dimensional, must also be questioned ((1), p. 31). The results with which she substantiates this view are subject to the same errors (varying length of exposure, and the time error), and seem to be the more doubtful, since the ascription of bodily quality to the AI, and surface quality to the MI, stands in such obvious contradiction to the facts of ordinary observation. Gottheil ((3), pp. 87 ff.) attempts rather unconvincingly to rescue the case by postulating a gradual reversal of the principle of depth in images in adult life, during which time it is contended that the MI takes over the more primitive functions of the AI and the EI.

Failure to establish the principle of depth as a criterion for distinguishing the three types of images does not, however, invalidate the importance of depth as a descriptive attribute. It is important to note that the EI of a three-dimensional object is almost never devoid of bodily character, and that the EI of a two-dimensional object (*e.g.* a picture), especially among clear-cut cases of eidetic ability, frequently appears in marked relief ((13), p. 47).

6. *Size of the Visual Field*. Experiments were set by Busse to determine the area of the ground which falls within the field of vision simultaneously with each of the three types of image, or more specifically, to determine at what distance from the centre of the image a peripheral stimulus is effective[1]. In six out of nine cases the area of the visual field thus determined was for MI > EI > AI, and in the remaining three MI = EI = AI. The occurrence of equal visual areas in 30 per cent. of the cases makes it impossible to consider this criterion as decisive. Furthermore, as Koffka rightly points out, the longer and more rigid fixation required for the arousal of the AI would account sufficiently for the narrowing of the visual field ((11), p. 129).

7. *Coloration*. Among 35 *Eidetiker* Herwig found 21 whose EI were habitually positive (*i.e.* in the colour of original) and 14 whose EI were

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[1] It seems incongruous that the Marburg investigators, after directing their observers to produce a MI by 'thinking and not seeing' should find it possible to locate with accuracy the 'centre' of this image on a projection mat, and in their experiments generally to treat the MI as a mensurable, 'perceptual' phenomenon.

habitually negative (*i.e.* complementary in colour) ((5), p. 147). He considers it significant that the positive EI seems to be a characteristic possession of the younger observers, and that with increasing age the EI tend to become complementary in colour. Under certain conditions (*e.g.* long exposure and direct fixation) those observers who customarily report positive EI may be able to secure only negative EI ((5), p. 151). From these facts Herwig concludes that the positive EI is biologically an unstable phenomenon, and that under unfavourable experimental conditions, as well as in later life, it tends to resemble more and more the ordinary negative AI ((5), pp. 208 ff.).

To those observers who customarily report positive EI the colours in the image are often more 'brilliant' than in the original[1]. Such observers are able to hold both the object and the image in the field of vision for simultaneous comparison of colour qualities ((8), p. 51)[2]. In these cases the colour appears to be like 'frozen gas,' without object character. This *Flächenfarbe* is especially characteristic of the EI which appears when the eyes of the observer are closed ((13), p. 45). On the other hand the colours are readily projected on to, and bound up with, the 'surface' of the image whenever the observer's fixation is more rigid, or when he is set to regard the image as an object of true perception (*i.e.* when he is *dinghaft eingestellt*) ((13), p. 44; (12), pp. 88, 107). Thus the problem of the manner in which the colours of the EI appear becomes largely one of the influence of attitude, and, as has been suggested in the discussion of the localization of the EI, does not differ essentially from the problem of the appearance of colour in the AI.

There are cases in which the EI appears only with brightness differences and without chroma ((5), p. 132). There are also cases, and these are undoubtedly of common occurrence, where the EI of pure colour without form appears. Adults without other evidence of eidetic ability frequently report that they can at will suffuse a neutral surface, or when the eyes are closed, the retinal grey, with any colour they choose. I can find no discussion of this phenomenon in the Marburg work, but it unquestionably belongs to the important, but as yet relatively uninvestigated, category of spontaneous EI.

8. Among the functional characteristics of the EI the first to be considered is the *Conditions of its Arousal and Disappearance*. The points on which there seems to be reasonable agreement are the following:

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[1] Cf. S. C. Colvin, "A marked Case of Mimetic Ideation," *Psychol. Rev.* XVII, p. 261, 1910.

[2] Cf. J. Ward, *Psychological Principles*, p. 176.

(i) A longer time of presentation is required for the arousal of the EI than for the MI, and a longer time for the AI than for the EI. (Strangely enough the absolute length of presentation used varies greatly with different investigators. Thus Gösser uses for the AI the same presentation time as Busse for the EI, viz. 15 seconds. Cf. (11), p. 136. I have found the following periods to be most suitable: MI, 5 sec.; EI, 35 sec.; AI, 45 sec.) (ii) Rigid fixation is usually necessary to the arousal of an AI; but less often required for the EI, and practically never required for the MI ((4), p. 98; (1), p. 9). (iii) The MI is largely subject to voluntary recall, the EI to a less degree, and the AI not at all ((7), p. 9 *n.*; (11), p. 137). (iv) In a few cases clear EI are obtainable only when the eyes are closed; in others only when a light grey projection mat is employed; but "for the majority of cases a rather dark homogeneous grey, whose brightness can be compared approximately to that of the retinal grey, proves to be the best projection ground" ((13), p. 13)[1].

In this connection it is interesting to note the manner in which the EI appears. It sometimes develops while the object is still in the centre of the visual field, and in such cases appears as a 'transparent veil' before the object; in other cases it develops one or two seconds after the object is removed, and in still other cases a much longer time is required for its complete arousal. At times the details of the image develop simultaneously and at times successively. A number of observers report that the picture of a lion, for example, gives rise to an EI in which the head of the beast is clearly seen before the rest of the body ((13), pp. 24 ff.).

The EI may pale gradually until it disappears, or it may disappear piecemeal as separate features drop out, or it may vanish of a sudden when some disturbance of attention occurs. Voluntary suppression of a persistent EI results either in gradual paling or, what is truly remarkable, in forcing the image toward the periphery until it vanishes from the field of vision ((13), p. 26).

9. *Selective Tendencies*. Kroh cites from his records the fact that more than 50 per cent. of his *Eidetiker* failed to secure an EI from a picture representing an ordinary house but were successful in securing the image from a picture of a monkey. From this and from similar

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[1] Some adults who are unable to secure an EI of any object in rest frequently report a lively EI when they have watched for considerable time some small object in rapid motion ((13), p. 38). This phenomenon differs from the ordinary visual after-image of movement in that the *form* of the moving object is at all times clearly apprehended. Children often report vivid EI at night after a visit to the cinema—particularly, as they say, of the 'exciting parts' of a picture.

results he concludes that "it is plainly nothing else than the greater interest which one object possesses in comparison with another which gives it preference" in the arousal of an EI ((13), p. 31). This selection on the basis of interest or affective tone Jaensch designates as 'philotropic.' There is likewise a tendency for 'natural' objects and colours to arouse EI more readily than 'unnatural' ones ('ontotropic' tendency). Still another selective tendency, the aesthetic ('kalotropic') occasionally operates to restrict the EI to content which for the observer has aesthetic value ((13), p. 33).

Influential as these selective tendencies are one must not lose sight of the fact that the EI is frequently concerned with material which can have little intelligibility for the observer. Among my own subjects, for example, I found that a number of children whom I examined were able to spell out correctly, or almost correctly, from their image the German word *Gartenwirthschaft*, which was for them quite meaningless. The exposure of 35 seconds was not sufficient to permit a 'learning' of the word, especially since the picture itself was filled with incident and detail of lively interest which the child had likewise to describe. The essential features of the picture were described with more fluency, to be sure; but upon being pressed to 'observe' their image more closely each child was able, often to his own surprise, to 'see' the small letters over the door. Three of my 30 *Eidetiker* spelled the word correctly; 7 had not more than two errors; and only 5 failed to give at least five letters correctly. In all cases the letters were given with equal accuracy, whether read from left to right or from right to left. I do not argue that foreign words have no interest for the child, but simply wish to point out that such detail is extraneous and relatively meaningless, and is not retained by virtue of any inherent structuration or organization with the remaining content (cf. discussion of the criterion of Clearness).

10. *Flexibility*. Although the EI tends to reproduce accurately the content of its model there are not infrequently cases where this content undergoes striking alterations. The child may change the position or character of some of the details, or he may attribute detail to his image which is entirely lacking in the model. In both cases the child declares that the new features are actually 'seen' in the image, and he is generally unable to recognize them as innovations. Such changes occur especially when the image is recalled after a lapse of considerable time. The alterations are always fundamentally consistent with the child's actual experience; he is definitely unable to introduce into his image features which are at variance with the normal constitution of things ((13), p. 35).

This close relationship between associative (memory) factors and the eidetic image is made much of by the Marburg school. It is invoked as proof of the original 'undifferentiated unity' of all types of imagery (*vide infra*, p. 113).

Besides being subject to such unconscious alterations the content of the EI is to a certain extent under voluntary control. My observers who reported well-defined EI were all able, by an effort of attention, to cause movement within the image. Thus a carriage was made to drive away, turn a corner in the road, and so to disappear entirely from the image. People could be made to enter and leave, and to perform *normal* actions. The range of flexibility is very great indeed, but it does not extend to include the ridiculous or unnatural. It is well known that changes in the content of the AI are not subject to voluntary control, whereas the MI is almost completely a voluntary phenomenon[1]. In this respect then the EI must be regarded as more nearly related to purely central (ideational) imagery ((5), p. 142).

11. *Coherence.* Gösser asserts that images of the different 'memory-levels' are to different degrees 'coherent' with the perceptual data which are to be found simultaneously within the visual field. The higher the level the less is the 'coherence' between the image and the perception: MI < EI < AI ((4), pp. 125 ff.). The evidence in support of this contention is of several kinds. (i) There is first the alleged tendency of the AI to 'fasten' itself on to the projection ground after the manner of the *Oberflächenfarbe*, and of the EI to 'free' itself from the ground after the manner of the *Flächenfarbe*. This phenomenon has already been considered (in the discussion of Localization), and was shown to be in the last analysis a problem of attitude. (ii) When the projection ground is folded or bent the AI tends to accommodate itself after the manner of the picture cast by a lantern-projector against a rough wall. The EI, on the other hand, may or may not accommodate itself to the unevenness; and the MI remains totally uninfluenced. (iii) In much the same way the colour of the ground forces an alteration in the colour of the AI, so that a true colour mixture takes place; the MI remains entirely uninfluenced by the colour of the ground. The EI occupies a middle position: occasionally its colour blends with that of the ground (as in Gösser's experiments with the EI of a red postage stamp, projected against a blue stamp as ground ((4), pp. 118 ff.); but more usually there is a rivalry or a very incomplete mixture ((4), pp. 127 ff.). (iv) When the image of a vertical ellipse is projected on to a ground which can be rotated, it is

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[1] Cf. W. James, *Principles of Psychology*, II, p. 50 *n*.

found that the angle of rotation required to cause the image to change its elliptical shape for a circular shape is for MI > EI > AI. Here again the proposition seems to hold that the MI is least, and the AI most, affected by the behaviour of the ground. This type of experiment has been employed by several investigators ((4), pp. 110 ff.; (1), p. 22); but the results are not convincing. The angle of rotation necessary to produce a rounding of the image is for different individuals much greater than for the different types of images (cf. (11), pp. 140 ff.). Furthermore, several cases are recorded by Krellenberg ((12), p. 105) where the EI and the AI grow narrower rather than rounder as the projection ground is rotated. *A*-typical cases in all such investigations are usually explained by the Marburg school as belonging to the so-called 'Unitary Type.' For this type there is said to exist only one undifferentiated type of imagery, and that bearing a univocal correspondence to the perception proper ((7), p. 18; see also discussion in the following section of this paper).

12. *Plasticity.* By the degree of plasticity of an image is understood the extent to which it may be influenced by the conditions of a preceding experiment ((1), p. 14). The Marburg investigators endeavour to minimize this factor by maintaining, in those series in which they deal with all three types of images, the same experimental sequence, viz. MI, EI, AI. Taken in this order the influence of the preceding members of the series is said to be less than in any other possible sequence ((12), p. 64). It is, however, impossible to exclude entirely, or even to correct for, the influence of the previous attitude. In certain cases this influence constitutes an important source of experimental error (cf. (11), pp. 138 f.). The EI, always the object of chief interest, is placed in an unsuitable and anomalous position in the middle of the series, and to what extent its attributes are a result of perseveration in the MI it is impossible to determine. It has already been shown (in the discussion of *Clearness*) how an MI which is inaccurate in regard to certain detail will cause a subsequent EI to be inaccurate in regard to the same detail.

13. *Invariability.* The EI does not seem to conform, as does the AI, to Emmert's Law (that the size of the image varies proportionately with the distance of the projection ground from the eye) ((5), p. 144). It is true that the EI does tend in some cases to increase in size as the distance of the projection is increased, and the closer the EI resembles the AI in its general characteristics the more does this tendency approach true conformity to Emmert's Law ((13), p. 40). On the other hand, the closer the general resemblance between the EI and the MI the less variable is

its size[1]. The EI then, according to its nature, approaches either the AI or the MI in respect to Invariability. The distances which have been employed in these experiments cover only a small range, being limited by most investigators to 50, 100 and 150 cm. Even within these limits the invariability of the AI is seldom exactly zero (*i.e.* seldom is an exact proportion preserved between the size and the distance), and the irregularities in the behaviour of the EI and MI are numerous (cf. (11), pp. 144 ff.).

14. *Displacement in Space.* The position of an object in space seems to have little influence upon a young child's perception of its form. It is relatively indifferent to him, for example, whether his picture book is in its right position or turned upside down. In drawing, the child frequently confuses right with left, vertical with horizontal, and up with down[2]. He may reproduce form with considerable accuracy and still lack appreciation for its position in space (*i.e.* relative to himself). This phenomenon has its exact counterpart in eidetic imagery ((9), p. 123; (13), p. 39). Images are frequently reversed, turned upside down, or displaced from right to left. Unfortunately little control has been attempted for this strange phenomenon, and at present it offers no points of comparison with the displacement of perceptions or of memory images.

15. *Fusion.* Jaensch reports an interesting experiment in which it was possible to produce a composite or generic EI. Several leaves of the same species of plant (snowberry), each showing unmistakable individual differences of shape, were laid in a row before the observer. He was instructed to look intently at the first until an EI was aroused, then to project this image upon the second leaf, and so on until he came to the end of the series. "There appeared at the end of the series for a large number of observers a unique type of synthetic image; it was not a static image, but one which evinced continuous movement and alteration. First appears a leaf with smooth edge, then indentations occur which become deeper and deeper until the contour of the most deeply indented leaf is reached or even exceeded. Then the indentations become more shallow until the original smooth form is attained. The *Fluxion* repeats several times" ((7), p. 29). Kroh states that one-fourth of all the juvenile *Eidetiker* under his investigation showed this type of synthetic EI. Other types of fusion have been studied, with only two

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[1] The MI by its very nature tends always to preserve the same proportion as exists with the perception ((3), p. 79); see also Fechner, *Elemente der Psychophysik*, II, p. 473.

[2] W. Stern, *Psychologie der frühen Kindheit*, pp. 147, 288.

objects. The result in these cases is always a synthetic EI which possesses truly composite properties of size and form ((13), p. 39)[1].

III. '*Memory-levels*' *and* '*Undifferentiated Unity.*'

When we ask what may be the function of eidetic imagery in mental life and why it should be an almost exclusive possession of children we are confronted at once with an ingenious genetic theory. This theory, propounded first by Jaensch, rests upon a conception of a progressive series of 'memory-levels.' "The transition from one of these stadia to another is continuous, so that the eidetic image now resembles more closely the memory-image and now the after-image" ((4), p. 97). The experimental evidence, though at times unconvincing, tends on the whole to justify this view; or, more cautiously stated, the evidence shows that in respect to those attributes which are shared by all three types of images, the EI tends to assume a middle position between the AI and the MI.

The Marburg investigators, however, mean more than this by the doctrine of 'memory-levels.' It is for them a genetic as well as a descriptive fact. Memory, says Busse, is a purposive, if not a structural unity ((1), p. 64). The AI, the EI, and the MI represent progressively higher states in a 'teleological continuum.' As the individual advances in years he comes to depend more and more upon the higher grades of memory, while the lower fall into disuse ((1), p. 65). Kroh ascribes to the AI the most primitive qualities and functions in the memory scale, but denies that it has chronological precedence, though this would seem to be implied in Busse's statement ((13), p. 117). Jaensch, on the other hand, goes farther than Kroh and regards the eidetic phenomenon not only as the original form of memory, but as the ontogenetic source of perception as well! ((7), pp. 18, 23). At the outset of his development the child has neither perception nor memory as distinct faculties; for both are simply aspects of a still more primitive type of experience. No doubt before there can be experience there must be sensory stimulus, but what such stimulus evokes is not first perception and then memory or first memory and then perception, but rather a primitive and undifferentiated conscious content, an EI. This content has the capacity for *creating* perceptions out of the manifold of sensory material which comes to the child as he makes his contacts with the environment ((1), p. 58). As the needs of the individual change with his development there gradually evolve from the original eidetic experience true perception and true memory with all their distinctive attributes ((12), pp. 56 f.; (7), p. 23; (13), pp. 118 ff.).

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[1] The significance of these findings for the theory of the origin of generic images is not far to seek. Cf. Galton, *Inquiries into Human Faculty*, 2nd Edit., p. 132, and Binet, *Psychology of Reasoning*, Transl., p. 118, 1899.

This hypothesis, according to Jaensch, is to be defended by two types of evidence—first, the nature and extent of eidetic ability, and, secondly, the information available concerning the thought processes of primitive peoples, particularly the tendency which they manifest to react, not to perceptual data alone, or to ideas alone, but as it were to both together. The stimulus situation is totally undifferentiated ((7), p. 23).

(i) At least 50 per cent. of all children possess a greater or less degree of eidetic power. A large number of the remaining 50 per cent., says Busse, show some rudiments of the ability ((1), p. 58). Now a certain number of children who possess marked eidetic imagery may be classified under the 'Unitary Type,' that is to say, their images of all three 'memory-levels' show a marked similarity in appearance and in behaviour ((12), p. 57). There does not seem to be a sharp break between this type and the ordinary eidetic type, but the child who is truly of the 'Unitary Type,' according to Krellenberg, "cannot produce negative AI; even with a protracted fixation he is able to secure only an EI in natural colours....Likewise he is unable to produce true MI, for these pass over immediately into EI. The original Eidetic Unity is likewise close to perception, and can often be interchanged with it" ((12), p. 117). Krellenberg draws for support chiefly upon his experiments regarding the criterion of Invariability. His records, however, show that none of his selected cases were of the ideal type just described ((12), p. 63). In short, the existence of this remarkable type, where all experience is eidetic, remains completely unproved.

Furthermore, before assuming that the original form of memory is the EI more satisfactory evidence should be secured of the universality of the ability among young children. It does not seem to be the case that the power diminishes in proportion to the advance in years, or that very young children as a whole have better eidetic ability than older children. Statistics tend to show that the high point of the ability is reached in children from 12 to 15 years of age (cf. (11), pp. 161 ff.). It is possible, of course, that the difficulty of securing reliable introspection from young children may make the estimates for the early years too low. But the evidence in hand cannot support a theory which must rest upon a universality of eidetic imagery among young children. And if the theory of the 'Unitary Type' is to be taken literally it must be proved that all young children not only have eidetic imagery, but that they have nothing else than eidetic imagery!

(ii) The second line of evidence, which concerns the behaviour and ideas of primitive peoples, is likewise inconclusive. It consists chiefly of anecdotal material which illustrates abundantly the well-known animism of primitive belief; but which scarcely convinces one that the origin of this animism is to be found in the "undifferentiated unity of an original eidetic world" ((6), pp. 88 ff.; (13), pp. 137 ff.). The problem is strictly ontogenetic and cannot be solved by an appeal to the metaphysics of primitive peoples.

IV. *Theoretical Note.*

Images present so many points of similarity with their corresponding perceptions that psychology has only with difficulty evolved criteria for distinguishing between them[1]; and yet if in the adult mind these spheres were not totally distinct, a fatal confusion would beset the adjustments of the individual. In order to explain how it is possible for images to retain a resemblance to original impressions and at the same time to become safely differentiated from them, it has been suggested many times that the transitional stages of imagery be examined. Some have said that the key to the solution is to be found in a better understanding of the nature and functions of the AI[2]. But after-images belong to sensation, and have nothing whatever to do with memory. Eidetic imagery would seem to have a more legitimate claim to be the connecting link. Certainly on the descriptive side the claim is justified. Besides the evidence which has been presented in earlier sections of this paper there is the opinion of many psychologists who in former times have recognized the eidetic phenomenon (under various names) as an intermediate stage in the continuum of images. The following quotations, for example, are particularly interesting:

> "The most immediate phenomena of imagination would seem to be those tardier images—due to what the Germans call *Sinnesgedächtniss*—coercive hauntings of the mind by echoes of unusual experiences for hours after the latter have taken place."
> —James, *Principles of Psychology*, II, p. 44.

> "Distinct as they are, impressions and images are still closely connected. In the first place, there are two or three well-marked intermediate forms, so that, though we cannot directly observe it, we seem justified in assuming a steady transition from one to the other. As the first of such intermediate forms, it is usual to reckon... after-images; still further removed from normal sensations...are 'recurrent sensa-

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[1] For a discussion of these criteria see Bertrand Russell, *Analysis of Mind*, pp. 145 ff.

[2] Cf. S. I. Franz, "On After-Images," *Psychol. Rev. Monog.*, No. 12, pp. 51 ff., 1899. The Marburg psychologists likewise speak of the AI as the lowest stage of a progressive series of *Gedächtnisbilder* ((1), p. 65).

tions';...finally what Fechner has called the 'memory after-image.'"—Ward, *Psychological Principles*, pp. 174 f.

"It seems to me that the *Erinnerungsnachbild* is a matter of special interest inasmuch as it seems clearly to show how the same phenomenon in respect to individuality and circumstance can oscillate (*schwanken*) between the character of the *Erinnerungsbild* and of the *Nachbild*."—Fechner, *Elemente der Psychophysik*, II, p. 497.

The mere fact of resemblance between the various types of imagery does not prove that memory and imagination have evolved through lower stages. On the contrary, memory, whether conceived in terms of recognition, or of disposition to repeat responses, seems from the beginning to exist as an independent phenomenon. Not all children have eidetic images, and yet all have memory. Furthermore, the child is able to make a clear distinction between what he *remembers* and what he '*sees*' in his image. It is true that there may at times be a substitution of the EI for the MI, as in the case of individuals who have the ability to visualize whole pages of written material without the slightest effort at 'learning' (13), pp. 121 f.); but such ability is always co-extensive with memory *qua* memory. As for imagination, the same principle seems to hold. Eidetic imagery is only one form of imagination, and exists during childhood along with the ordinary 'reproductive' and 'productive' varieties of imagination. The eidetic phenomenon is then an intermediate form of imagery in the sense that it shows at one and the same time characteristics which pertain to both the sensory and the ideational spheres; but it cannot be proved to be a transitional stage in the evolution of higher forms of imagery from lower. Its significance for the mental life of the child must be sought in a different direction.

Memory-images, it has been pointed out, are 'well-structured' or esemplastic representations of previous experiences in which the salient features are made prominent and irrelevant detail obscured or unrepresented. Blended with the visual picture are motor impulses and affective tendencies which give the image its significance as an agent in bringing about adaptive response[1]. If memory-images were nothing more than copies of impressions our mental life would be an inextricable chaos of photographically accurate records. Such a state would not facilitate the organizing, fusing, abridging, and interchanging which enable the individual to vary his reaction; an image too closely bound to a specific previous situation would tend inevitably to stereotype his modes of response.

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[1] The particular function of the MI, according to Bartlett, is "to manifest itself when the reaction is hesitating, and to lead to a response which is decisive." This *Journal*, XI, p. 333, 1921.

The child, on the other hand, whose reactions are less predetermined by habit, and who requires not so much to vary his behaviour as to react appropriately to the concrete elements of a given situation, is benefited by an image which represents more literally the actual sensory detail. A single fleeting presentation is not enough to enable the young child to derive the full 'reactive significance' from the stimulus. He craves to be shown the same picture book again and again, and to be told the same story repeatedly, without the variation of so much as a word. It is only by virtue of such ceaseless repetition during this 'sucking-in' period that the child can perfect his adaptive responses, and so make of them the material for his attitudes and habits of later life.

The EI seems to serve essentially the same purpose in the mental development of the child as does the repetition of a stimulus situation. It permits the concrete 'sensory' aspects of the surrounding world to penetrate thoroughly into his mind[1]. The young child delights in conjuring up his images: a parade of soldiers, a circus, a train journey, or even a trivial domestic scene may haunt him for days or weeks, furnishing him material of great interest for his play activities. It is sometimes only with difficulty that he is persuaded to distrust the reality of these vivid images. He reacts to them with the same degree of seriousness as to a genuine stimulus situation: he is terrified by his image of a wild animal; he is contented for hours with the companionship of his imaginary playmate; and he insists dogmatically upon the reality of his most fanciful visions. Such pseudo-sensory experience enables him to 'study out' in his own way and in his own time the various possibilities for response contained within the stimulus situation. His reaction when the situation is first presented is often incomplete, the presence of adults, or the lack of time, preventing him from becoming thoroughly acquainted with its properties. A period of reflection is necessary, during which he may experiment in various ways with his image, varying his behaviour to conform sometimes to one and sometimes to another aspect of the situation, gradually gaining a comprehension of the full meaning of the whole, and building up the attitude which is to determine his future response to the same or to analogous situations.

It may be objected that the EI does not always preserve the stimulus situation in true detail, and that it would not be advantageous for the child to base his reactions upon a false image. Sometimes the alterations which occur within an image seem to be detached and sporadic, apparently resulting from accidental associations; sometimes they seem to be consciously controlled, as in the case of a child who alters the events of the play-yard, the classroom or the cinema so as to supply desired elements in an autistic wish-fulfilment[1]; and in some cases the child seems to create an EI at will—lively illustrations, for example, to accompany a story which is being told. In all of these cases, even in those which seem to be acts of 'pure imagination,' the images are still concrete, still rich in stimulus value for the child. The flexible images furthermore never make impossible demands upon a child. Innovation or new combinations which involve contradiction with experience are excluded. As a rule the child simply blends into his image a certain amount of relevant associated material; or in the case of the spontaneous EI he constructs from his previous experience an image (strictly *erfahrungsgemäss*) as a substitute for a situation which is not actually present. In any case these altered images no less than the simple mimetic images enable the child to base his responses upon a concrete pseudo-sensory situation. The only difference seems to be that while the persistence of the literal image enables him to perfect his responses to a situation when the original stimulus has ceased to operate, the alterations in the images enable him to *elaborate*, within limits, the stimulus situation, and so to provide himself with data for a certain versatility in behaviour.

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[1] It is, of course, assumed that both the repeated stimulus situation and the EI here referred to are concerned with material which has *interest* value for the child; otherwise the repetition and the image would have no significance whatever.

The reason for the retreat of the eidetic ability during adolescence seems to be fairly obvious. The general adjustments of the child to the concrete aspects of his environment are well determined by the time puberty sets in. His experience has been broad, and his responses so often repeated that a mere skeleton memory-image alone suffices to facilitate his reactions. Interest in this period of life shifts from the tangible world to inner experience of the emotional type. The emphasis upon abstract thinking in higher schools undoubtedly helps to destroy the youth's reliance upon his eidetic images. There is no longer a fertile soil for their cultivation. Whether or not there are also physiological reasons for the disappearance has not yet been determined[2]. In later life there is no need for detailed images; indeed unless they were held strictly under control they would be a serious liability. Individuals in whom the ability endures seem frequently to be predetermined to literary or artistic vocations ((13), pp. 127 ff.); but in other cases the EI of the adult becomes obsessive in character and prejudicial to normal adjustment ((13), pp. 185 ff.). In any case eidetic imagery is an anomaly in adult life. Its true function is performed only in the earlier years of mental development, when by preserving and elaborating sensory data it enhances the meaning of the stimulus situation for the child and enables him to perfect his adaptive responses.

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[1] A discussion of eidetic imagery in relation to day-dreaming is to be found in Kroh's work ((13), pp. 137 ff.).

[2] Possible physiological correlates of eidetic imagery have been discussed by W. Jaensch. See, *e.g. Uber psychophysische Konstitutionstypen*, *Monatsschrift f. Kinderheilkunde*, XXII, pp. 1-10, 1921.

V. *Summary.*

1. The eidetic image is a unique psychical phenomenon. It differs from the ordinary visual memory-image in that (*a*) it possesses a pseudo-perceptual character, *i.e.* it is definitely localized in visual space even though recognizable as a subjective phenomenon; (*b*) it is generally superior in clearness and richness of detail; (*c*) its clearness is less dependent upon 'structuration' or organization in its content; (*d*) it is generally more mimetic, *i.e.* more accurate in its reproduction of detail; (*e*) it is generally more brilliant and more accurate in coloration; (*f*) it requires more rigid fixation for its arousal; (*g*) it is more dependent upon a favourable projection ground for its arousal and shows a greater degree of 'coherence' with this ground. It differs from the ordinary after-image in that (*a*) it may be aroused by a complicated and detailed object; (*b*) it is superior in clearness and richness of detail; (*c*) it continues longer in the visual field; (*d*) it is subject to voluntary recall after the lapse of considerable time; (*e*) it requires a shorter length of exposure and less rigid fixation for its arousal; (*f*) it is more dependent upon factors of interest and 'naturalness'; (*g*) it is subject to voluntary control and can be made to change its content by an effort of attention.

2. It tends to *resemble* the MI in respect to its 'associative' characteristics. In fact it behaves very much like a purely central image in that (*a*) the content is to a considerable extent selected according to its affective (interest) value; (*b*) the content can be altered within the limits of experience by an act of will; (*c*) the content is influenced by preceding images. It tends to resemble the AI in respect to its 'physiological' characteristics, for (*a*) it appears always in visual space; (*b*) when it is held in the field of vision there is a marked tension in the muscles of the eye; and (*c*) it may be either positive or negative in coloration.

3. The differences as well as the resemblances show variations in degree. At one time the EI seems to lie closer in its general characteristics to the AI and at another time to the MI; and yet the points summarized above afford adequate ground for regarding the EI as a distinct

phenomenon. The fact that it is transitional in character does not vitiate its individuality.

4. Eidetic imagery is a common possession of children; the ability usually retreats with advancing age, but vestiges often remain in later life, and occasionally well-marked cases of the *eidetische Anlage* are met with among adults.

5. The Marburg theories as they stand are not acceptable, for they rely for support upon aspects of the eidetic phenomenon concerning which there is great uncertainty.

6. The function of the EI seems to be to preserve and to elaborate a concrete stimulus situation for the child in such a way as to intensify for him the sensory aspects of experience. By so doing it enhances for him the meaning of the stimulus situation and enables him to repeat and to perfect his adaptive responses.

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One circumstance which has undoubtedly tended to deter inquiry from the field I have in view ought to be referred to at the outset. As psychologists, we are compelled to accept what is expressed by the terms retention and revival as an ultimate condition of mental life. We have, that is to say, no means of getting behind the facility thus designated, and of exhibiting the grounds of its possibility. Attempts to do this have, it is true, been made; but the results of such attempts no one can pronounce encouraging. What, for example, is gained by speaking of psychical phenomena as being subject to a law of 'mnemic causality,' in contradistinction from the law of physical causality that holds of physical phenomena? Does it amount to more than using new terminology to describe that which eludes explanation? And the like is true of the phraseology which Bergson is fond of employing. What is really gained by speaking of a conscious state as being 'permeated' or 'interpenetrated' by others that have preceded it, unless some definite notion of the *modus operandi* of the process thereby indicated be forthcoming? For these phrases are in themselves in no sense explanatory; they are but pictorial ways of re-stating that which calls for explanation. Nor do I think that in the present connexion much help is afforded by the analogies to which Hering and other physiologists have pointed in the behaviour of nervous tissue and of organic matter generally. It may, no doubt, be safely asserted (*a*) that the original perception which is, as we say, revived, itself involved or depended upon certain changes in the organic conditions, and (*b*) that the revival involves or depends upon changes partially, at all events, similar to those which accompanied the original perception. And there would appear to be ample justification for the further assertion that the whole complex of organic conditions does not remain uninfluenced by such changes but that in truth the network of organic processes comes, by dint of repeated modifications, to be altered in its structure and to respond in a fashion different from what it otherwise would have done to any stimulation similar to that which occurred previously. Accordingly, it is natural enough to look upon 'organic habits,' so-called, as a sort of provision already made in the bodily organism itself for the retentiveness and the revival of experience that is characteristic of the mental life. And I am not in the least anxious to question the important part that may be, and in fact must be, played, in this respect, by the bodily mechanism. But, in the first place, the extremely limited extent of our knowledge of these organic conditions precludes any but highly hypothetical conclusions being based upon them. We do not even know what physiological processes are involved in having the original experience which is afterwards revived—much less, therefore, the kind of modification they undergo by repetition. And, in the second place, the completest knowledge of the organic conditions would not in itself enable us to form any conception of what it is that is conditioned by them. It is, in truth, only on the analogy of 'habit' as exemplified in the mental life, that we are enabled to speak of 'organic habits' at all; and, for a matter of that, the analogy utterly breaks down in regard to the one feature which from the psychological point of view is the most significant.

We have, then, to acknowledge that we have no psychological explanation to offer of the fundamental fact of retentiveness or revival. But we *can*, when trying to deal with the matter psychologically, avoid introducing into our descriptive account metaphorical expressions which are readily seen to be not only erroneous but positively mischievous. I will not dwell upon such hopelessly unscientific and misleading figures of speech as 'calling up a visual (or other) image,' although they are still frequently employed in a way seeming to imply that the phrase 'calling up' is an exact and literal representation of what as a matter of fact actually takes place. I am thinking rather of expressions such as 'residua' and 'traces' (the equivalent of Aristotle's term *μοναί*), which, if they mean anything, point to a mode of operation on the part of the mind for the existence of which there is not the slightest evidence, and which suggest conceptions of the structure of the mental life wholly irreconcilable with what we now do know of its nature. It is, as Mr Bradley once said, mere mythology to talk of a 'copy,' which a presentation has sloughed off, persisting in some sub-conscious region of the mind, there awaiting in disconsolate exile, until association announces resurrection and recall; or, as Professor Ward puts it, of images or representations being accumulated and "somewhere crowded together like shades on the banks of the Styx[1]." Even the less objectionable term 'disposition' used in such a context as that presentations leave behind them dispositions for revival, can only be allowed to pass provided it be understood to carry with it no theory of what is actually taking place when a presentation is no longer, as current phraseology has it, 'in consciousness.'

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[1] It is surprising that Aristotle's doctrine of *φαντασία*, or *τὸ φανταστικόν*, has lingered on, in the way it has done, down to the present time. Aristotle conceived that *αἰσθήματα*, sense-impressions, do not perish when the act of perception has ceased to be. They leave, he thought, traces (*μοναί*) of themselves, and these are somehow stored up 'within us.' Through the exercise of the faculty of *φαντασία*, and by means of the *μοναί*, images or *φαντάσματα*, are brought into clear consciousness. There is much in Aristotle's psychology that would be of value to the modern investigator; but it is unfortunate that this particular doctrine of his should have re-asserted itself so persistently.

My purpose in the following pages is to set down the outlines of a theory, to which by various lines of reflexion I have been recently led, of the nature of the content presented in and through the act of imagining. In conformity with what I have just been contending, I shall proceed on the assumption that retentiveness is an essential characteristic of the mental life; or, in other words, that the mental life, so far as we are familiar with it, invariably evinces a certain degree of continuity, that it is not confined to what is directly experienced at the moment, but that, in some form, what has been continues to be. How the continuance is effected I shall make no attempt to determine. The theory I am about to advance appears to me to have a considerable body of evidence in its favour, and to be compatible with the facts that can be fairly said to be established. But I am putting it forward tentatively, and in the hope of eliciting criticism which may either assist in its further elaboration or suggest a better way of attacking the problem. For, in a matter of this kind, even a wrong theory may be of service, in so far as it directs attention to an inquiry that might otherwise be indefinitely postponed.

I.

A sufficiently safe proposition from which to start may be expressed in the form that imagination is continuous with perception and grows out of it. Few modern psychologists, at any rate, would, I conceive, be willing to acquiesce in what seems to have been J. S. Mill's position that the difference between a 'sensation' and an 'idea' is an 'ultimate and primordial' difference. It is worth noting that even Mill himself apparently accepted the description of an 'idea' as a 'consequence' of a 'sensation'—a 'consequence' which, although it can be distinguished from the 'sensation' and treated as something different from it, 'is yet more like the sensation than anything else can be,' in fact so like that it can be called a copy or an image of the sensation[1]. Apart from the many awkward questions that might be pressed in respect to the term 'copy,' it is enough to urge, in the present connexion, that if one thing is said to be a 'copy' of another the implication is that some definite relation of dependence subsists between the former and the latter. And, in truth, the dependence of imagination upon perception is rendered obvious by several very ordinary considerations. For one thing, imaginative activity is clearly limited, and strictly limited, by what has already been apprehended through the instrumentality of sense. Be the powers of 'productive imagination' never so great, they do not enable us to create for ourselves an absolutely new colour[1], or an absolutely new sound. Moreover, even so far as the ordering and arranging of the material is concerned, the facility of combination and separation is confined within certain circumscribed boundaries. If two features have been invariably conjoined in perceptive experience, we cannot represent them to ourselves as apart. Thus, while we may imaginatively vary at pleasure the colour of a material object, it is not possible for us to imagine it without any colour at all. "The liveliest imagination is not able to picture a fairy-world which is not so constructed that its constituents can in essence be brought under the categories of physical things. The task of giving an exhaustive scientific description of so chimerical an entity may be difficult; but the difficulty does not prejudice the principle; any 'castle in the air' whatsoever has its architecture[2]." *Prima facie*, therefore, there would appear to be abundant justification for regarding perception as the primary process and for making it the starting-point in any discussion of imagination.

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[1] See James Mill's *Analysis of the Human Mind*, I. p. 52 and J. S. Mill's Notes, pp. 68–69 and pp. 402–423 (especially pp. 412–418).

Nevertheless, the reverse method has sometimes been followed (as, for example, by Berkeley), in which case it is, as Professor Alexander has observed, impossible not to be struck with the spectacle of mental activity, so that, wellnigh without misgiving, 'images' are taken to be purely mental products; and thence, coming to perception, the temptation is strong to interpret its data as mental entities likewise. A good illustration of the procedure is to be found in Taine's treatise *De l'intelligence*, a book that in many respects is still of psychological value. Beginning with what he calls 'images' and the laws of their revival, Taine proceeds to view perception as in fact a process of imagining which turns out to be in harmony with external things[3]; instead of describing a hallucination as a false external perception, we ought, he argues, to describe an external perception as a true hallucination. In short, what in perception we are immediately aware of are, he maintains, just images, or 'internal phantoms,' which present the aspect of external objects. The objects we touch, or see, or perceive by any one of the senses, are nothing more than semblances precisely similar to those which arise in the mind of a hypnotized person, a dreamer, or an individual subject to illusions. No more need here be said of this doctrine than that it completely fails to account for the extraordinary assumed fact of 'internal phantoms' appearing as external objects at all.

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[1] Hume's hypothetical case of a possible exception to this rule (in the first section of Book I of the *Treatise*) is, as he says, "so singular, that 'tis scarce worth our observing, and does not merit that for it alone we should alter our general maxim."

[2] Münsterberg, *Grundzüge der Psychologie*, p. 341.

[3] One can hardly help surmising that Bergson, in some of the arguments of *Matière et Mémoire*, must have been considerably influenced by Taine.

The analysis which in various publications[1] I have sought to give of the nature of perceptive activity is based upon a radically opposite principle—the principle, namely, that in perception the knowing mind is directly apprehending given external fact. I have tried to shew that perception consists neither in the passive reception of what is given nor yet in merely contemplating the given, but that it is essentially an act of discriminating, of distinguishing, and of comparing the features or characteristics of the given. Such misunderstandings as have arisen respecting the analysis, as I have described it, have, so far as I can judge, been chiefly due to the circumstance that the critics have not realised the fundamental significance one is attaching to the *function of discriminating*. If once the notion of discriminating were taken seriously and the implications it carries with it recognised, I suspect that many of the difficulties which some writers have felt in following the exposition would disappear. To put the point as emphatically as I can, I will venture upon a bold analogy. Just as for Kant the act of synthesising *was* the very act of knowing, so I would maintain that the act of discriminating *is* virtually the act of knowing, or, at all events, the fundamental characteristic of that act *qua* act. It has, however, to be remembered that, in the history of mind, discrimination exhibits the most varied stages of development—starting with the crudest possible distinction of that which appears as an obscure somewhat from the vague indefinite background, and extending to the deliberate use of ideas of relation, such as we are familiar with in conceptual thought.

Let me briefly re-state the whole position in a manner somewhat different from that in which I have hitherto stated it. The *occurrence* of any act of perception, the occasioning condition of its *existence* as a state of the mental life, is, I have allowed, undoubtedly traceable to the physiological event of bodily stimulation; its *character* or *nature*, on the other hand, is not thus to be accounted for, but is explicable only by viewing it from within and as in relation to that upon which it is directed. The distinction between existence and essence or content, between the 'that' and the 'what' of any concrete fact is as old as Aristotle; and, thanks to Mr Bradley, it has become sufficiently familiar in current philosophical discussion. Now, in the situation we describe as 'perception of an object,' two concrete facts are involved—on the one hand, the given object and, on the other hand, the act or process of perceiving it. Each of these concrete facts exhibits, then, the two aspects of existence and content. But, in view of what occurs in this situation—the gradual discrimination, namely, by the conscious subject, of the content of the object—a further distinction is here requisite. It is requisite, that is to say, to distinguish that which I have been in the habit of calling the 'content apprehended' both from the content of the object and from the content of the mental act. To bring out the import of this distinction, Professor Ward's well-chosen illustration (employed by him, however, in a different context) of bestowing in the course of a few minutes half a dozen glances at a strange and curious flower will serve. Assuming that the act of apprehension is directed upon the actual flower, as a concrete fact in the external world, we may assert that the cognising subject will gradually discriminate a multiplicity of its features—at first the general outline, next the disposition of petals, stamens etc., afterwards the attachment of the anthers, position of the ovary, and so forth—he will, in other words, become aware by degrees of a variety of features constituting the content of the flower. And this *awareness* of the features of the flower is not, it will be conceded, something that can be severed from the act of being aware—*i.e.* the act of apprehending. If one describes it as the content of the act of apprehending at a particular stage of its progress, or as that which gives to the act in question its specific character and enables it to be distinguished from other cognising acts of the same conscious subject, one will be doing no violence either to language or to the facts. No one would wish to maintain that this *awareness* is that which in the instance we are considering is cognised, that *it* is the object upon which the act of apprehension is directed. No one would, I should suppose, wish to deny that such awareness is a characteristic of the act of apprehension, when that act has reached a certain measure of completeness. In contrast with this, the 'content apprehended' is that which is frequently designated the 'appearance' of the object to the percipient. It, likewise, is not the object upon which the act of apprehension is directed. For the object is, *ex hypothesi*, the actual flower—an object which the conscious subject gradually comes to recognise has a variety of characteristics—shape, size, colours, etc. But the sum of the characteristics which the conscious subject will be aware of at any specific moment will be different from the sum of characteristics which he will be aware of at another moment, and either of these will only be a fragment of the much larger sum of characteristics which there are good reasons for believing the flower itself possesses. Clearly, therefore, the sum of *apprehended* features (*i.e.* the content apprehended, or the 'appearance' of the object) is *distinguishable* from the larger sum of characteristics constituting the *whole* content of the object. Just as clearly the former cannot be an existent fact, be it called a 'presentation,' or a 'sense-datum,' or what not. For it is, if one may use the term in this connexion, a selection from the features forming the content of the object, and we have already premised that the content or nature of any concrete fact, such as a flower, is not to be confused with its existence, that its 'what' is distinguishable from its 'that.' So far, then, from this selection of features being there, as an existent fact, prior to the act of apprehension, and in some way calling forth that act, it only comes to be in virtue of the act of apprehension having been first of all directed upon the actual object and apart from such act would have had no 'being' of any sort.

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[1] See, *e.g.*, *Proceedings of Aristotelian Society*, N.S. vol. XVII, 1917, pp. 318 *sqq.* and pp. 434 *sqq.*

One other point is worth emphasising. In the threefold distinction just insisted upon, the term 'content' has been employed quite consistently and unambiguously. Throughout it has signified a sum of characteristics. The content of the given thing is the sum of its characteristics or properties; the 'content apprehended' is, we may say provisionally, so many of those characteristics as are, for the time being, cognised; and the content of the act of perceiving is the sum of those characteristics of the said act which is described as awareness of the features just referred to.

When we go on to deal with the facts of revival or retention, the importance of the distinction between 'content apprehended' and 'content of the act of apprehending' becomes at once apparent. For, if the foregoing analysis be on the right lines, it can alone be the *awareness*—the content, namely, of a mental act—that is capable of being revived or 'reproduced.' The 'content apprehended' cannot itself persist after the act through and by means of which it has its being has ceased to exist. It cannot persist in and for itself, simply because it is not an existent, not what Dr W. E. Johnson calls a substantive proper, not, that is to say, either a continuant or an occurrent[1]. And it cannot persist in the mind, because it has never been 'in' the mind, in the strict sense of the term[2]. On the other hand, the contents of our own cognitive acts, the awarenesses, if one may be permitted so to name them, which we live through, or *erleben*—these are the mind's own property, or rather go to constitute its very being, and these we are forced to recognise it has the power of retaining in some form and of reviving. Even they are not, of course, to be conceived as isolated, independent entities. Even they are not brought back again as objects, simply because they never were objects. Nor are the specific acts or states of mind themselves literally 'reproduced' or 'recalled,' because an act or state of mind is an event or an occurrent, and an event or occurrent can only happen once. But, in accordance with what we have agreed to accept as for psychology an ultimate fact, we can say that it pertains to the very being of a mind that it has the facility, in and through subsequent states, of 'reproducing' or 'recalling' the awareness which was the content of a previous state and of utilising this retained awareness in the life of the present and of the future.

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[1] *Logic*, vol. II. pp. xi–xii. Cp. vol. I. pp. 199–200.

[2] The only sense in which it can be said to be 'in' the mind is that it is present *to* the mind, or apprehended *by* the mind.

The next point to insist upon is that no act of perception occurs in our mature mental life without being immensely influenced by the facility just alluded to. In dealing with perception so far I have treated it, and for the sake of simplicity it was allowable so to treat it, as though it took place on each occasion *de novo*. The conclusion reached as regards its essential character will be in no way invalidated by now introducing the factor one had deliberately neglected. The act of perceiving remains from first to last an act of discriminating and of thus becoming aware of the features of its object. But that process is enormously furthered by the circumstance that it takes place in a mind which by long and repeated practice has come to perform such acts more or less habitually and by aid of the facility of retention or revival. The well-worn illustration which Stout cites from a book of Hutchison Stirling's will here suit my purpose[1]. When one fine morning a ship unexpectedly appeared on the horizon, *what* it was was evident at a glance to Crusoe. Yet what to Crusoe was a ship was to his man Friday only an amorphous blur, a perplexing, confusing, frightening mass of detail, which would not assume for him the form of a definite, coherent object. There was, that is to say, a tremendous difference between the contents apprehended by these two individuals confronted though they were by one and the same object. The external conditions were similar; the dissimilarity between what they respectively perceived was largely traceable to their previous mental histories. Crusoe had seen ships scores of times before, and a revival of his former awareness came at once to his aid.

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[1] *Manual of Psychology*, 3rd ed. p. 38.

What he was actually discriminating at the moment was probably far less than what Friday was discriminating, and yet Friday was at a loss to make out what the mysterious thing out there could possibly be. The instance is a typical one. In ordinary perception, there can be no question, a vast deal of what we suppose ourselves to be immediately discerning is not, as a matter of fact, immediately discerned; it is discerned through the aid of the revival of previous awareness of similar objects. In other words, the perception of a mature mind is interpenetrated[1] with what accrues to it from a long series of former perceptive acts.

Thus, as the mental life develops, our apprehension of things tends to become less and less immediate and direct. The contents of what we call our knowledge, of what we are said to know about objects, gradually come to assume the form of an inward possession, constituting almost an instrument wherewith we proceed further to differentiate and to grasp the nature of the world to be known. Consequently, in the case of a familiar object—and the great majority of the objects we encounter are familiar—we do not require on each occasion to discriminate afresh its manifold characteristics. The act of perception is certainly directed upon that object, but its familiarity saves us from the necessity of going through the whole process of discriminating anew. It is enough that we discriminate at the moment only a relatively small number of its features; these immediately suggest the awareness of features previously discriminated; and the apprehension in question is attained with an ease and rapidity that would otherwise have been impossible. We have here, in fact, an example of that economy of labour which consciousness throughout its procedure exemplifies. In short, our perception tends to become less and less dependent upon what, at the time, is actually given; we bring to bear upon what is given a wealth of awareness which ensures that no perceptive act is ever, even in its incipient stage, devoid of specific content. Furthermore, it is precisely this wealth of accumulated awareness that constitutes what we are in the habit of describing as our experience; and in a very real sense it can be said that it is its experience which *makes* a mind. This, I take it, is part, at any rate, of Bosanquet's meaning in insisting that "mind is always a world." "There is," he says, and quite truly, "nothing to be called 'the mind' of which there are empty acts exercised upon objects[2]." In themselves, acts of discriminating, regarded in abstraction from the wealth of accumulated content, would form so much psychical mechanism, as Lotze styled it; but when we speak of a mind, it is invariably the totality of its experience to which we are referring, and not merely to the mechanism of its modes of activity.

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[1] Bergson's term may perhaps be used in this context without the risk of misunderstanding.

[2] *Three Chapters on the Nature of Mind*, p. 20.

II.

The term 'imagination' is no doubt a vague term covering a great variety of processes. But there is, so far as I can see, no valid objection to employing it, in a general way, to include both 'reproductive' and 'productive' imagination; and no useful psychological purpose that I can discover is served by confining it, as some writers would do, to the latter of these, and re-naming the former 'imaging.' For, in the first place, as we shall see, 'reproductive' imagination is not always 'imaging'; and, in the second place, we certainly require a term to denote that which is common to both these modes of mental activity. Taking the term, then, in this wide sense, I shall now try to shew reason for holding that the process of imagining is, in truth, of one piece, so to speak, with the process of perceiving, as we have viewed it, the chief difference being that in imagination a relatively larger proportion of revived factors are involved.

It is easy to make the transition from the one process to the other by means of instances in regard to which this is manifestly true. An imaginative child is, let us suppose, gazing at a mass of fleecy clouds in the play of the sunlight. Soon the shapes and forms of its various parts will assume for him the appearance of chariots, and horses, and warriors, like a scene in ancient legend. The child will, that is to say, be apprehending the given object through the medium of his revived experience of pictures, story-books, tales to which he has listened, and so forth. J. M. Barrie has somewhere tried to indicate what a map of a child's mind would be like, and singles out, amongst other things, the recollections of savages and lonely glens, and gnomes who are mostly tailors, and caves through which a river runs, and princes with six elder brothers, and a hut fast going to decay, and one tiny old lady with a hooked nose. Some such map would account for what happened on that midnight journey with the beehives which Tess took with her small brother Abraham, in Thomas Hardy's novel. The little fellow, as he more fully awoke, so the novelist relates, "began to talk of the strange shapes assumed by the various dark objects against the sky; of this tree that looked like a raging tiger springing from a lair; of that which resembled a giant's head," etc. And even for Tess herself "the mute procession past her shoulders of trees and hedges became attached to fantastic scenes outside reality, and the occasional heave of the wind became the sigh of some immense sad soul." Most people could, I suppose, supply parallel instances from their own experience. There is here, to put it briefly, as there is in perception, a certain nucleus, if we may so express it, of actually discriminated fact, although no doubt less than what is usually discriminated in normal perception. And round this nucleus of actually perceived fact, there is, in consequence of the revived awareness suffusing, as it were, the act through which the discrimination takes place, a penumbra, so to speak, of features that seem to share with the nucleus the characteristic of objectivity. That a large number of so-called 'images' which appear to stand over against the conscious subject as objects are thus susceptible of explanation is, I take it, scarcely open to question. Even those who maintain that images are "physical facts forming part of the world of physical things[1]" would hardly, I should think, wish to contend that, in the neighbourhood of the trees against the dark background, there were veritably in the external world 'images' of tigers and of giants' heads, upon which Abraham's perceptive activity was directed. At all events, it would generally be admitted that here, round the nucleus of actually discriminated fact, features due to the boy's revived awareness of tigers and giants, which he had previously either seen in pictures or heard accounts of, gathered, and led to his interpreting the given objects in the way he did. For such imagery, then, we have an explanation that certainly looks as though it were a correct explanation. I believe, and shall attempt to shew, that it is a mode of explanation which may be extended to a variety of other cases where its applicability is less obvious. The gist of the explanation, it will be observed, is not merely that sense-stimulation is involved, but that in imagination, where objective imagery is present, there is, as in perception, a real object upon which the act of discriminating is directed, and that this accounts for the objective character which the content apprehended seems to possess, although the number of the features of this object actually discriminated is considerably less than in perception, and the portion of the apprehended content traceable to revived awareness considerably greater, and more arbitrary and haphazard. We must be prepared, no doubt, to recognise that bodily factors, and not only extra-organic things, may, in some situations, function as objects.

One proviso must, however, be insisted on. In speaking, in familiar language, of 'images' of imagination, people are wont to group under this term things which are, in truth, radically different. And one fundamental difference that is of great importance in the present connexion is this. Some so-called 'images'—certain visual and auditory images more especially—seem to stand over against the apprehending subject, and to be for him objects in space, no less decidedly than the physical things of nature seem to stand over against him and to be for him objects in space. If, for instance, one is reclining on a sofa in a state of fatigue and relinquishing oneself to idle reverie, as it is called, it is a familiar experience that, without any deliberate resolve on his part, visual images (say) of faces, sometimes little resembling those of people he has known, flit before him, and that these appear to be stationed in space not far distant from the visual organ itself. But, on the other hand, when we talk in particular of 'memory-images,' it is quite certain that we often do not mean definite objective pictures of this kind. I can remember, for example, what stood on my breakfast-table this morning, and enumerate the different articles there accurately enough, without having any definite picture of the breakfast-table standing over against me at the present moment. Galton's well-known researches carried out in 1875, with respect particularly to visual memory-images, established once for all the fact that there is extraordinary divergence among individuals in the power of what he described as visualising. While the great majority of the men of science to whom he applied protested that so-called 'mental imagery' was unknown to them, he came across numerous cases where visual images, in the sense indicated, were especially vivid. And he concluded that, on the whole, such imagery was more frequent in childhood than in adult life, that it was more frequent in persons engaged in some kinds of occupation—artists, for example—than in persons engaged in other kinds of occupation, and that it was almost entirely absent in the case of those occupied in mental work involving abstruse generalisation and abstract thought. Memory is not, then, necessarily dependent upon concrete objective imagery. There can be memory—and memory of a very reliable kind—without any representation in the form of an 'image' of what is remembered. Here, no doubt, verbal or other signs are extensively, if not always, employed; and these would be, for the most part, objects, in the sense I am understanding by the term. I do not propose further to discuss this sort of memory; it is clearly explicable on the lines I have indicated.

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[1] Cp. J. Laird, *A Study in Realism*, p. 67.

It seems to me that the term 'images' ought to be reserved for such contents as in memory and imagination do appear to stand over against the cognising mind as objects, and upon which the act of apprehension seems (to the conscious subject in question) to be directed. At all events, it is with 'images' of this description[1] that, in the following pages, I shall be alone concerned. And the view of them which I shall endeavour to base upon empirical grounds is, I repeat, that there is always, in their case, a nucleus, however scanty and concealed it may be, of something actually present to the senses upon which the act of discriminating is initially directed, and that around this nucleus a penumbra of imagery gathers, owing to the circumstance that the act of apprehension which is directed upon the real object is saturated, so to speak, with revived or retained awarenesses of the nature already described.

The interesting series of experiments conducted by Mr F. C. Bartlett[2] furnish abundant evidence of the way in which 'images' are called forth by actually perceived objects. In the last series, in which the method of ink-blots, variously shaded or coloured, on cards, was adopted, the subjects were instructed to see what they could make of them, after the manner in which they sometimes found shapes for clouds and saw faces in a fire. "It is in relation to that which *might* be many different things," Mr Bartlett writes, "that images most readily arise, although at the same time the image tends to be of something in particular." He says of one of his subjects that he "'projected' his image on to the blot"; but it is not quite clear, from his account, how far the different blots actually *assumed* for the other subjects the forms which they *suggested* to them. Presuming, however, that in many cases they did, the results fully accord with what one would have anticipated. The enormous variety of the imagery of different persons occasioned by the same object was an outstanding feature. What to one person was a 'camel' was to another person a 'tortoise,' to another 'a dog worrying a table-cloth,' to another 'a baby in a cot with a doll falling out,' to another 'a picture of Sohrab and Rustum in a book of Arnold's poems,' and so forth. The way in which the special interests of the persons in question came into play was again strikingly apparent, as also the tendency of a certain type of imagery, once suggested, to persist when one object after another was presented to the same subject. But our problem is not so much to discover the kind of imagery that may be occasioned by specific objects as to discover, if we can, the kinds of objects involved in various types of imagery.

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[1] That such 'images' do occur, and in the case of certain individuals with great frequency, is, I think, indisputable. Dr L. Arnaud Reid's attempt to sustain the proposition that "there are no images, but only *imaging* of real things" (*Knowledge and Truth*, pp. 112 *sqq.*) seems to me to break down in the face of overwhelming testimony to the contrary.

[2] See this *Journal*, VIII. pp. 222 *sqq.*

Probably one may speak most confidently in regard to images connected with the lower senses. Images of taste, for example, occur in conjunction with sense-perception—the image of a sour taste may be occasioned by the sight of an unripe apple; or a hungry man, seated before a savoury dish, may have an image of its flavour;—but it would, I conceive, be extraordinarily difficult to make out a plausible case for these images occurring otherwise. So, too, olfactory images may arise when, for example, one is looking at a scentless carnation and represents to oneself the pleasant odour of old cloves, or the mention of castor oil awakens an image of a peculiarly disagreeable smell; but, again, I think it would be excessively difficult to provide an instance of olfactory images where no obvious object is present. Once more, of tactile images the same may be said. "The most vivid touch-images come," according to William James, "when we ourselves barely escape local injury, or when we see another injured"; and he quotes an instance given by Meyer of an educated man, who, at the moment of his fright at having accidentally crushed the finger of one of his little children in the door, felt a violent pain in the corresponding finger of his own body, that remained with him for three days[1]. Meyer further relates that he himself could excite images of warmth, cold or pressure upon any part of the skin; and that they might become so lively as to cause him to pass his hand over the place just as if it were a real impression on the skin.

Kinaesthetic imagery is, it need hardly be said, vastly more common than that just alluded to, but is perhaps the least accessible of all imagery to exact inspection. In this reference, it is to be remembered that perception, as it occurs in the concrete life of mind, is by no means a merely cognitive process but involves a change in the state of feeling and, as resulting from both, a certain form of movement. Consequently, every mode of sense-perception has a tendency, more or less marked, to give rise to kinaesthetic imagery; and, in the revival of such sensory experience, kinaesthetic imagery will be naturally involved. Another circumstance also calls for notice. In the mature mental life, motor presentations have lost their originally chaotic and haphazard character, and fall into regular groups and series. And, in such groups and series, it is certain that motor images repeatedly take the place of motor sensations. Here, too, it seems to me, it would hardly be possible to produce instances that are incompatible with the view I am taking. Those whose imagery is predominantly of the motor type tend to represent things either in terms of the movements they would themselves be prone to make in the presence of the things in question, or in terms of the words they would use in naming and describing them. The frequency with which familiar objects induce motor imagery is notorious. On glancing at one's pen, for example, there may arise the image of grasping it for the purpose of writing, on touching our bunch of keys the image of the movement necessary for unlocking a safe, and so forth. So, too, nothing is more common than a verbal image aroused by an object the name of which, or of something suggested by it, we wish to recall. The instances given by Stricker[1], whose imagery was mainly of the motor type, and whose treatment of the subject is of special value, all point in a similar direction. Stricker's imagination of movements of any objects whatsoever appeared to be paralysed when there was no actual experience of movement either in his own eyes or in his own limbs. For the purpose of imaging a soldier marching, for example, he would begin to march himself; and he even succeeded by practice in inducing his eye-movements to 'act vicariously' for his leg-movements in imaging men walking. He shews, too, how dependent verbal imagination is on actually perceived movements in lips, tongue, larynx, etc., and Professor J. B. Watson has recently been enforcing similar considerations from the point of view of 'behaviourism.'

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[1] *Principles of Psychology*, II. pp. 65–66.

It is, then, with respect to auditory and visual images that our theory must be chiefly tested. And here, no doubt, the evidence is more liable to dispute. But I believe the reason largely is that, in regard to auditory and visual imagery, it is extremely difficult to draw the line between images which are objective—images, that is to say, in the strict sense—and those which are not. When, for instance, people are said to possess the gift of "internal hearing," or when they are said to "see with the mental eye," it is not, in a great number of instances, at any rate, implied that they have definite objective images, but that they are capable of introspection in an eminent degree—in other words, of reflecting upon previous mental states of theirs. On the other hand, when a tune is said to be heard in imagination with as much distinctness as in the original perceptual experience, or when Coleridge relates that in the course of a profound sleep "all the images (of Kubla Khan's palace) rose up before him as *things*," we are concerned, I take it, with presented content of quite another description.

Auditory imagery of the objective type is certainly far less frequent than visual imagery of that type, but it is, for all that, by no means infrequent. Taine reports, for example, of himself that, in thinking over a representation of the *Prophète*, he could repeat silently the pastorale from the overture, and follow, almost feel, not only the order of the notes, their different height, rests and lengths, not only the musical phrase repeated as an echo, but also the keen piercing tone of the hautboy which played it. He adds that every good musician experiences this sort of thing at will, when he follows the lines of music covered with their black marks. Yes; "when he follows the lines of music covered with their black marks." And the question I am pressing is whether there is not always something corresponding to these "black marks" when auditory imagery assumes the form it is thus described as, in these cases, possessing. There are persons who declare that after having listened to a performance of the Passion Music of Bach they can recall at will without difficulty in the silence of their homes exactly what they heard in the concert-room. But they can generally be driven to admit that in such recall they are not conscious of actual melodies in the external world, and that the memory-images, vivid though they may be, do not compensate them for being unable to attend another performance of the same music. I surmise, therefore, that what they are in truth reproducing in such cases are the contents of their perceptive acts whilst in the concert-hall. And the imagery that results may well cluster round some nucleus of perceived fact, *e.g.* sounds which even in the stillness are present, or the voice of a friend, or some visual datum, and thus acquire the objectivity which it displays.

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[1] *Studien über die Bewegungsvorstellungen* (1882). Cp. his *Studien uber die Sprachvorstellungen* (1880).

A variety of circumstances account for the fact that the contents apprehended through vision come to have assigned to them by the perceiving subject a *pre-eminently* objective character. For one thing stimulation of the eye is ordinarily accompanied by a relatively small amount of organic affection and of distinctively marked modes of feeling-tone. In the great majority of cases it is wholly impossible to detect any more of the element of feeling-tone than is involved merely as a consequent of alteration in the momentary state of the bodily organ. And comparative immunity from this prevailingly subjective factor renders visual perception more transparently apprehensive of objective fact than it would otherwise appear to be. Another circumstance making for the same result is that we can discriminate with remarkable precision a visually apprehended content from the movements of the eye itself, so that the visual content stands in decided contrast to the eye-movement, which is only in a secondary fashion recognised as being an objective process at all. Moreover, visually perceived contents connect themselves readily with the objective characteristics determined by tactual and motor experience, because, of all sense-data, they afford the best opportunity for apprehension of a number of features simultaneously. Naturally, then, we should expect to find visual imagery of a more pronouncedly objective type than is found in imagery connected with the other senses. And this is, in fact, what we do find. From the accounts which good visualizers have furnished of their experiences instances innumerable might be cited in confirmation of the theory I am advancing. A few must here suffice. An English painter, quoted by Taine[1], explains his mode of work in the following manner: "When a sitter came, I looked at him attentively for half an hour, sketching from time to time on the canvas. I wanted no more. I put away my canvas, and took another sitter. When I wished to resume my first portrait, I took the man and sat him in the chair, where I saw him as distinctly as if he had been before me in his own proper person—I may almost say more vividly. I looked from time to time at the imaginary figure, then worked with my pencil, then referred to the countenance, and so on, just as I should have done had the sitter been there—*when I looked at the chair I saw the man.*" Abercrombie[2] asserts that a friend of his had one day been looking intensely at a small print of the Virgin and Child, and on raising his head beheld at the further end of the apartment (upon which his attention must have been directed) a female figure of the size of life with a child in her arms, the figure corresponding exactly with that which he had contemplated in the print. Taine[3] mentions the case of a whole ship's crew being thrown into consternation by the ghost of the cook, who had died a few days before. "He was distinctly seen by them all, walking on the water with a peculiar gait by which he was distinguished, one of his legs being shorter than the other. The cook, so plainly recognised, was only a piece of old wreck." Another case he quotes is also significant—that of a man, seventy-five years old, of sound mind, who came home one day frightened by a thousand phantoms, which were pursuing him. "Whichever way he looked, *objects were transformed into spectres*, representing sometimes huge spiders which ran at him to drink his blood; sometimes soldiers with pikes." When a bandage was applied to his eyes the visions ceased but returned as soon as the bandage was taken off, until the patient kept it on uninterruptedly for a night and part of a day. Then he saw phantoms only at long intervals, and after some days they disappeared entirely. These are extreme examples. But they illustrate the kind of thing I am supposing to take place where it is less easy to detect it. Even in such experiments as those of G. H. Meyer[1], who by practice succeeded in making it possible for himself to call up visual imagery at will with closed eyes, the presence of an actual object, and a visual one, is not excluded. For, not to mention the entoptic phenomena to which I shall refer later on, the black background is still an object. The phenomena of synaesthesia might also be used in support of the contention, *e.g.* the interesting cases of 'coloured hearing,' described by Dr C. S. Myers[2]. Audible notes, namely, suggest colours, which for one of Dr Myers' subjects varied with the pitch of the notes.

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[1] *On Intelligence*, p. 45.

[2] *Inquiries concerning the Intellectual Powers*, p. 63.

[3] *Op. cit.* p. 60.

In the make-believe of children ample evidence can be shown of the way in which the realisation of imagery takes place in connection with sense-perception. "The stump of a doll, woefully unlike as it is to what the child's fancy makes it, is yet a sensible fact, and as such gives support and substance to the realising impulse[3]." Sully remarks that the impulse to invent imaginary surroundings, and more especially to create mythical companions, is very common among lonely and imaginative children. I will take as an instance a small boy of my acquaintance who, when he was between the age of three and four years, invented for himself two imaginary playfellows, whom he christened respectively Binny and Nurny. Until he reached the age of six or seven years, he was constantly to be found amusing himself with them and conversing with them. He would speak to them through the window, while he was being dressed in the morning (*e.g.* "Is that you, Binny? Stay a second; I shall be down in a moment."), or he would exclaim, as his mother was about to take her seat at the breakfast-table, "Oh, *please*, not that chair! Don't you *see* that Nurny is sitting on it?" thus obliging her to seek another. Unquestionably these imaginary little creatures were for him distinct objects; but invariably they were present as in intimate connection with real things,—either seated on chairs, or walking about in the garden, or standing by the counter in a shop, and so on. Actual sense-perception, that is to say, was necessary in order that the make-believe should be realised or come to pass.

III.

Some striking confirmation of our theory can be obtained from a consideration of the imagery of the dream-life. Certainly not all dream-imagery can be said to be of the objective type. It is perfectly possible to go through the most vivid dream-experience without being able to discover in what one can remember of it on awaking any objective imagery, at any rate of the visual or auditory kind. But we are here concerned with dream-imagery that is of the objective type. And let it be noted at the outset, as one of the most characteristic features of the night-dream or dream proper, that there is unhesitating acceptance on the part of the dreamer of the contents of his dream-images as real. The conditions under which these dream-images occur are such as to deprive the individual of the possibility of exercising that criticism of what is presented to him which he habitually does exercise in the normal waking life; and, in the absence of all means of distinguishing the real from the imaginary, he takes every apprehended content to be the content of a real entity. *For the dreamer*, that is to say, the whole realm of the imaginary is literally wiped out—a fact which, so far as I know, has never yet had justice done to it in the rapidly increasing literature of "dream psychology."

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[1] *Untersuchungen über die Physiologie der Nervenfaser*, pp. 238 *sqq.*

[2] This *Journal*, vol. IV. pp. 228 *sqq.* and vol. VII. pp. 112 *sqq.*

[3] J. Sully, *Studies of Childhood*, p. 51.

It has long been recognised that at least a very large number of dreams originate in consequence of actual perception on the part of the individual, and several well-known writers have gone to the extent of saying that probably all dreams originate in this way[1]. Those who take this view are by no means necessarily committed to the assumption that "the primary cause of a dream is some stimulus[2]," any more than those who accept the ordinary doctrine that the primary cause of an act of perception is some stimulus are necessarily committed to the assumption that that stimulus is the primary cause of the content perceived. What the position does imply is, I take it, that the content, of which the individual is (in and through the mental act occasioned by the stimulus) vaguely and confusedly aware, suggests, or starts the suggestion of, those features that play the predominant part in the dream. The evidence which goes to shew that this is so could scarcely be stronger than it is.

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[1] "It is customary," writes Wundt, "to regard the majority of dream-phantasms as pure hallucinations. This view is hard to sustain. Probably most, perhaps even all, dream-presentations are in reality illusions, inasmuch as they are engendered by the slight sensory impressions, which are never extinguished in sleep" (*Grundzüge*, 4te. Aufl. Band II. p. 536). W. Weygandt (*Entstehung der Träume*) takes a similar view. Tissié (*Les Rêves*) declares that "dreams of a purely psychic origin do not exist"; and Havelock Ellis (*The World of Dreams*, pp. 71–72) holds that "most, perhaps indeed all, dreams that are sufficiently vivid to be clearly remembered on awakening, have received an initial stimulus from some external, or at all events, peripheral source." So, too, Bergson (*L'Énergie Spirituelle*, p. 99) asserts that "it is out of real sensation that we fabricate the dream."

[2] T. H. Pear, *Remembering and Forgetting*, p. 74.

It is hardly necessary to refer to those dreams, frequent enough, that are initiated through 'organic sensations' connected with the internal organs of the body. In consequence of the obliteration of the features that divert attention from them in the waking life, these 'organic sensations' attain during sleep a prominence in consciousness. Tissié tried to shew that disorders of digestion, breathing, and circulation can be correlated with definite kinds of dreams; and there are well attested instances of premonitions in dream-experience of serious maladies, such as heart disease and typhoid fever[1], owing to the circumstance that in the dream-experience the incipient symptoms are enabled to assert themselves more pronouncedly than in waking experience. So, too, the pressure of the bed-clothes, an uncomfortable position of any part of the body, obstruction of the supply of air, etc., have often been observed to be excitants of dream-imagery. A slight internal pain may be represented as the bite of a savage dog, a small difficulty in breathing may assume the form of a terrifying incubus such as afflicts us in nightmares, an involuntary extension of the foot may appear as a fall down some vast abyss. The familiar dream-experience of flying or floating through space takes its rise not improbably from a vague consciousness of the rhythm of movement in respiration.

Much of the visual imagery in dreams especially of the earlier part of the night is doubtless engendered by the entoptic phenomena to which Johannes Müller was one of the first to call attention. Müller tells us that he rarely failed to see, before falling asleep, 'with closed eyes and in the darkness of the field of vision' a great variety of highly illumined and coloured objects. From his youth he had noticed them, and had learnt how to distinguish them and their rapidly changing forms and movements from the peculiar images of dream-life. They rarely took the shape of recognised realities, but usually appeared as fantastic figures of men, animals and what not, such as he had never seen before. "I often follow these appearances," he says, "for a half-hour, until they finally pass over into the dream-images of sleep." Helmholtz, too, insisted on the importance of the phenomena in question. In his opinion every eye probably contains *muscae volitantes*, so-called; and of these fibres, granules, etc., floating in the vitrous humor, the shadows appear in the field of vision as little dark moving spots. And G. T. Ladd, who devised a method of experimenting with himself in order to determine the influence of what he describes as the *Eigenlicht* of the retina upon visual dreams, gives numerous instances of the actual transformation which he had observed of these "thin, pale, and almost senseless schemata" into the kind of things seen in dreams[1]. For example, several times on waking from a dream in which he had distinctly seen lines of printed letters forming words and sentences which he had been engaged in reading by sight, he clearly detected the minute light and dark spots, that had arranged themselves in parallel lines across the visual field, to which the illusion was due. There can hardly be any question that such "phosphenes" or "ocular spectra" do supply the objective nucleus of visual imagery in certain dreams. And I know not why they should not likewise to some extent be operative even in the waking life. Indeed, one can in a state of reverie, by fixing attention upon these little objects in the field of vision, watch the way in which imagery clusters round them.

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[1] Cp. Forbes Winslow, *Obscure Diseases*, pp. 611 *sqq.*, and Hammond, *Treatise on Insanity*, pp. 234 *sqq.* Even Aristotle (*De Div. per Somnum*, 463 a) notes that scientific physicians pay attention to the dreams of patients in diagnosing their symptoms not yet sufficiently evident in the waking state.

That numbers of external objects and events elicit the attention of a sleeper and set going trains of dream-imagery is matter of common observation. What would be to a person awake an insignificant noise, such as the sound of a bell or the fall of a tile, may to that same person asleep become a noise like thunder, and he may forthwith picture himself as being at the mercy of the elements and as witnessing a whole chapter of catastrophes. The creaking of furniture, the rain beating against the window, the howling of the wind—these are some of the sounds that are heard and that may be interpreted by a sleeper in all sorts of fantastic ways. Burdach and a companion pass a night during which there is a violent storm at an hotel, and they both dream that they are wandering in the darkness among high precipices. Havelock Ellis[2] relates a dream in which he was listening to a performance of Haydn's *Creation*, the orchestral part of the performance appearing to consist chiefly of a very realistic representation of the song of birds, though he did not identify the note of any bird in particular. Then followed solos by male singers, whom he saw, especially one who attracted his attention by singing at the close in a voice scarcely audible. On awakening, he discovered that the source of the dream was the song of a canary in an adjoining room. This instance is the more noteworthy because he had never heard Haydn's *Creation*, except in fragments, and it could only have been its reputation as regards the realistic representation of natural sounds that had been instrumental in occasioning the dream. So, too, external objects visually perceived constantly form the starting-point of dream-phantasy. Two cases quoted by Bergson[1] from Tissié's account will serve as illustrations. One person dreams that the theatre of Alexandria is on fire, that the whole place is lit up by the flame. Then he is transported to the fountain in the public square, and a line of fire is running along the chains which connect the great posts placed round the basin. After that, he is back in Paris at the Exhibition; it too is on fire, and he is taking part in the scene. On awaking, he finds that his eyes were catching the beam of light thrown by the lantern of the hospital nurse going her round. Another person dreams that he is in the navy, in which he had formerly served. Soon there is a battle going on and he beholds fire belching from the cannon. Again the dream is provoked by a beam of light from the lantern of a hospital nurse. A very common form of visual perception giving rise to dreams is that of bright moonlight and of the early sun's rays. An amusing instance is quoted by Radestock[2] of Krauss who once, when twenty-six years of age, caught himself, on waking, stretching out his arms towards what his dream-fancy had pictured as his mistress, but which turned out to be the full moon. And Radestock thinks it is not improbable that many of the dreams of celestial glory which persons of a mystical religious temperament are said to experience are occasioned by the rays of the sun or moon.

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[1] *Mind*, N.S. vol. I. 1892, pp. 299 *sqq.* The method consisted in keeping the eye closed on awaking, retaining the visual images of the dream for a brief interval, and observing them dissolve into the 'phosphenes' perceived by the waking consciousness.

[2] *Op. cit.* p. 82.

I submit, then, that we may fairly take the ways in which dreams are fabricated as a clue to what happens in the formation of 'images' strictly so-called in normal waking experience. In each of the instances just cited what happens is clearly analogous to what we noted as happening in the case of a child gazing at a mass of fleecy clouds, or in that of the boy Abraham on the occasion of the night-journey with his sister. And I would invite the reader to compare the instances I have given of dream-imagery in this section with the instances given in the previous section of imagery in the waking life. In point of fact, neither dream-experiences nor the working of imagination in the waking life can be understood except by reference to what takes place in normal perception. In normal perception, the sense-content directly apprehended serves as the pivot around which we group a mass of revived factors and concepts, interpreting the whole in objective terms. The standard of interpretation is our normal experience. The data presented will be misinterpreted if either they are themselves apprehended with insufficient precision or if the circumstances attending the apprehension of them are in any way unusual. If the quantity of the content presented be too small, if its elements be confused, if the time available for the discrimination of those elements be excessively brief, it must be a matter more or less of haphazard or chance what specific interpretation be put upon the data in question. And considerations such as these make manifest, I think, (*a*) how impossible it is to draw any fixed line between what we call imagination and what we call perception, and (*b*) the probability that in imagination as in perception there is actually given sense-material, for the most part vaguely and confusedly apprehended, and often, no doubt, intra-organic in nature, which serves as the pivot around which the suggested 'imagery' is grouped and hence interpreted in objective fashion.

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[1] *Op. cit.* pp. 93–94.

[2] *Schlaf und Traum*, p. 110.

One characteristic difference there *is* between perception and imagination, but it tends to strengthen rather than to militate against the view I am taking. In sense-perception, the exercise of attention increases the clearness and distinctness of the content apprehended. And such increase of clearness and distinctness is due to the increasing number of the features of the object that are differentiated by the perceiving subject and which are by him compared with, and related to, one another. But in regard to imagination a like rule does not appear to hold. On the contrary, direction of attention upon, or the effort to attend to, the imaginary object seems rather, in a large number of cases at any rate, to dissipate it, to have the effect either of causing it to disappear or of transforming it into something else[1]. It may, indeed, be urged that if this be so, the disappearance of the 'image' ought, on our theory, to result in the real object exhibiting itself, and occupying the place previously occupied by the 'image.' The reply is that, in numerous instances, this is exactly what does happen. If the child who discerns chariots and horses and warriors, when gazing at a mass of fleecy clouds, can be induced to attend more specifically to the actual shapes and structure of the clouds he will probably see them very much after the manner of his less imaginative parent, and the fantastic objects will vanish. And, similarly, in the case of the visual images that occur in reverie the exercise of attention results in the discovery of the actual objects. The reason why in other cases it does not is fairly obvious. In a large number of cases, the encroachment of penumbra upon nucleus will have been so great, and the latter will have been so confusedly and fragmentarily apprehended, that it is not surprising it should afterwards elude detection.

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[1] It is difficult at times to perform the experiment effectively because what we take to be an effort of attention may be simply an effort to hold the image in consciousness.

IV.

If our inquiry has been conducted so far along the right lines, we are confronted, it is true, at the end with a problem of fundamental importance. We have been admitting, namely, that in and through the act of imagination subjective factors influence to an extraordinary degree the appearance presented by what is really objective. What are actually revived or recalled are never, we have insisted, so-called 'presentations,' but contents of previous acts of perception, in other words, the awarenesses in which those acts have culminated. And the awareness of a characteristic, or sum of characteristics, is never, we have contended, itself an object; it never stands over against the individual subject as that upon which his act of apprehension is directed. How, then, does it come about that in the process of imagination these admittedly subjective factors do affect the 'content apprehended' in a most pronounced manner, and give rise to constituents in it that seem to the imagining subject to be unmistakably objective?

The problem is undoubtedly one that goes to the very root of the psychology of cognition, and it has, I think, to be confessed that we are not at present in a position to solve it. But, in seeking for a solution, some light may come from the consideration of certain phenomena, particularly in the sphere of aesthetics, which in late years have led to a considerable amount of discussion. Ruskin once spoke of a tendency of human imagination which, as he expressed it, "rejoicing in its own excessive life, puts gestures into clouds, and joy into waves, and voices into rocks"; and the term *Einfühlung*—empathy ('feeling into,' parallel with the term sympathy, 'feeling with'), as Professor Ward would render it—is but a technical name for that tendency. In its simplest form, *Einfühlung* consists in reading into impersonal and inorganic objects conscious feelings, emotions and desires which are really in ourselves. To borrow the hackneyed illustrations, the pinnacles of a Gothic tower may seem to us to be striving upwards, as we ourselves should strive against a load threatening to crush us; or we may attribute peacefulness to a gently murmuring brook, anger to the boisterous waves in a storm, seriousness to the movement of a melody in music. Poetry abounds, of course, with examples. Take, for instance, Wordsworth's well-known lines about the daffodils on the shore of Ulleswater,

> The waves beside them danced; but they
> Outdid the sparkling waves in glee:
> A poet could not but be gay,
> In such a jocund company.

In all such cases, the act of perception is markedly toned with subjective feeling, and the object perceived appears to be different from what it would appear to be were it apprehended in and through acts of perception not thus toned. The experience 'felt into' the object is at once an experience of the apprehending subject and seems to be a characteristic of the content apprehended by that subject; that which is peculiarly psychical is thus seemingly combined with the physical and other characteristics of the object to form a whole, after the manner in which these latter are combined with each other into wholes. Not only so. These psychical features are not merely 'thought into' the object conceptually; they are, so to speak, *seen* in the object. "The conscious subject," as Witasek[1] puts it, "represents intuitively (*anschaulich*) the psychical features expressed in the object by re-living them and inwardly perceiving them."

The relevance of all this to our present theme will be at once apparent. The feelings and other psychical factors involved in the process of *Einfühlung* are, in the terminology I have been using, contents, or partial contents, of mental acts; yet, inasmuch as the aesthetic object is contemplated in and through the mental acts of which they are contents, it wears the aspect of itself possessing them among its other properties. No doubt, among its other properties there must be some that are the inducing conditions of the affective characters which belong to the apprehending subject being thus ascribed to the object. But the essential point is that here those factors of the inner life which Hamilton designated 'subjectively subjective' are capable of influencing in a very prominent way the content apprehended. If this be true, then, of states of feeling, there need scarcely be hesitation in allowing a similar statement to be true of what we have called revived awarenesses. For these, although they are likewise contents of mental acts and are not, as such, objects, do, for the most part, refer, if I may meanwhile use the conventional phrase, to what is objective, and there is in so far the more reason why they should influence the content apprehended than states of feeling which have no such objective reference.

I am not for a moment suggesting that the comparison I have been making affords us any means of solving the problem which, as I have said, confronts us at the end of our inquiry. For in none of the extensive literature on the subject can I find any approach to a satisfactory explanation of the process of *Einfühlung* itself. All such phrases as 'externalisation,' 'transference,' 'projection,' 'projicience,' and the like, seem to me to be in this context purely figurative and metaphorical, and to help us not at all in understanding what it is that really takes place. But the circumstance that we have, at present, no satisfactory explanation to offer of the facts of *Einfühlung* ought not to prejudice us in recognising the facts themselves. And what I *am* suggesting is that the explanation, whatever it be, of these facts would in all likelihood enable us to solve the problem to which I have referred.

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[1] *Grundzüge der allgemeinen Aesthetik*, p. 132.

It may be worth while to add a few words upon one other point. The objection may be pressed that, in developing a theory of the nature of images, we have been forced to admit an influence of subjective factors that would fatally vitiate every act of *perception*. If, for example, in the illustration borrowed from *Tess*, the strange shapes of the trees, etc. were so readily transformed into phantasms, is it not possible that the subjective play of revived experiences may equally transform for us every object of perception? It is, I think, a fair rejoinder, that there is nothing, so far as is known, in the mode of operation of retention or revival that must necessarily lead to such vitiation. In any complex cognitive act, and an act of perception is obviously complex, we are, of course, exposed to manifold possibilities of error; and that we are perpetually liable to illusion in perception is notorious enough. The first vague and confused apprehension of an object must needs be defective; it is only in and through the refinement of the process which comes from repeated exercise that the chances of misinterpretation are by degrees reduced. And this general consideration applies in regard to the objection just mentioned. Undoubtedly, acquired experience may and often does lead us astray with respect to what we suppose ourselves to be immediately cognising. But advance in discriminative capacity, the consequent refinement and perfecting of that function, operates with respect to the revived factors precisely as it operates with respect to what is immediately cognised. We come gradually to a more and more accurate and circumscribed use of the revived factors; we learn gradually to turn them to relevant and exact account, we acquire a dexterity of rapidly selecting those that are appropriate and of utilising them in the right way. And then, so far from falsifying perception, revived experience adds enormously to its efficiency. It at once sets the perceptive activity free from having to go through a lot of work it would otherwise have to perform, and it supplies elements that further the process of discrimination itself. It is true that when the presented nucleus is diminished beyond a certain limit and there is a preponderatingly large penumbra due to retained experiences, the play of fancy may run riot. Yet, even so, the play of fancy does not, in our waking life, as a rule deceive us; we do not usually take it to be other than a play of fancy. As William James reminds us, the means at our disposal for distinguishing between real and imaginary fire, or real and imaginary water, are, in our normal experience sufficiently efficacious; so that "around the core of reality the world of laxly connected fancies and mere rhapsodical objects floats like a bank of clouds."

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INTROSPECTION AND THE METHOD OF PSYCHOLOGY[1]

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I.

DIFFICULTIES OF THE METHOD OF INTROSPECTION.

WHEN I began to study psychology I learned that introspection was its chief method, and this doctrine is still held in some prominent quarters. On the other hand we are told that introspection is no method at all, that, strictly speaking, it is in itself an impossibility, a delusion. The issue thus raised might seem fatal to the progress of a science, and yet psychology, unconcerned about this dilemma, seems to be steering its course with full sails set. A closer inspection, however, shows that psychology is pursuing courses that lie so far apart one from another that it is by no means certain whether they will eventually lead to the same end. Not long ago experimental psychology, the invention of the second half of the last century, appeared to be reaching the limits of its effectiveness, and when we consider that this very experimental psychology was mainly built up by introspection, we are forced to recognize that the methodological difficulty had very serious consequences for the development of the science.

The cardinal failing of experimental psychology was its remoteness from life. The longer experimental psychologists were at their work, the more did their results prove utterly inadequate for the solution of a number of problems which grew more and more pressing. The historian and philologist, the educator and psychopathologist found themselves continually beset by psychological problems which were neglected, or seemed even incapable of treatment, by experimental psychology; Comparative Psychology then appeared and opened up a new field of work with new methods, claiming that these methods were the only legitimate ones for ordinary human psychology also.

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[1] A paper read at a joint meeting of the British and Cambridge Psychological Societies held at the Psychological Laboratory, Cambridge, on Feb. 23rd, 1924.

Experimental psychology has not completely blinded itself towards this unhappy state of affairs. I may, as a pupil of Stumpf and Külpe, recall the effort of the Würzburg school to change the trend of psychology's development. But admirable as the task was, the Würzburg school did not achieve the success promised by its beginnings. The Würzburg method was a new and vigorous return to introspection, which was thus given more importance than ever before in experimental psychology. It might, therefore, seem as if introspection was the cause of the failure. The Külpe school, introspecting upon some simple thought-processes, found the associationist doctrine inadequate, *i.e.* it maintained that *functionally* the trend of our ideas could not be explained by the mechanistic interplay of associations, because *introspectively* the elements presupposed by the associationists, the images, played an absolutely unimportant rôle, while experiences of a totally different kind, imageless but meaningful, formed the core and frame of such processes. The objections raised against the Würzburg theory are well known. The observers, it is said, *mistook* their experiences, they interpreted instead of describing. Imageless experiences, the alleged thoughts, conscious attitudes, and so forth, were in reality nothing but a mass of half-developed images, competing for supremacy in consciousness; therefore only those laws were legitimate which governed these rising images, all the rest were unscientific speculations.

In this argument it is maintained that introspection can be mistaken, and even that, in some instances, introspection *cannot* be right, as all these 'latent' images, for example, cannot possibly be observed, but must deceive the observer into taking them for imageless contents. Consequently we need a criterion to distinguish between true and false observations, or between contents open to introspection and those which are not open to introspection. The criterion proposed is that such contents only should be introspected that are, for a sufficiently long time, clearly within the focus of attention. This is, in fact, the most fundamental of all the axioms of traditional psychology; what is not clear is, as such, unobservable, or can be observed only after it has been made clear. Consequently, all the descriptions of introspection are given in terms of clear content. The clearness, however, must be deduced from the description. If the content to be described is a part of a larger whole, it must nevertheless be focussed, be made the outstanding experience, no matter how insignificant it may be inside this whole. Just as when we cannot read a letter we turn on the light, or rely on the help of a magnifying lens, so, according to this way of psychological thinking, the light and the magnifying power of attention are utilized in introspection.

This analogy reveals the chief assumption of the doctrine under examination. We light up the room and use the lens because the object examined is not altered by these procedures. In the same way the introspective method under discussion supposes that a mere change of attention does not really alter the contents.

Mental content, then, is treated as if it were a material thing. But, whatever the mind is, it is not a mosaic of solid unalterable things. Here we are face to face with what is to my mind one of the basic errors of traditional psychology. On this error the whole system rests. This error is the justification of the selection we are to make among our contents, and it leads consistently to the fundamental concepts of sensation, image and feeling as *the* mental elements. Reality, as it appears to each of us, is to be reduced to these simple terms, although the richness of our world transcends these limits and the purposiveness of our own mind may oppose our efforts after simplification. As science built upon introspection ought to acknowledge these difficulties, by not doing so it vitiates its own principles, over and over again repudiating facts of introspection and explaining them away.

Introspection, then, has become bound up with *analytic* attention and this analytic attitude has still another aspect. We isolate contents from their own connections, and are forced to insert them into a new connection, namely, the permanent one of the subject, the Ego. Consequently the sensation is said to be 'subjective.' In introspecting colour, for instance, we are often told that we must not describe the property of a thing, but an element of our consciousness. If we compare lifted weights, the real psychological description, according to this view, does not say "This weight is heavier than that," but rather, "My sensation of tension is now stronger than before." In all this may be seen the same substantial way of thinking that has already been pointed out. No matter in what connection it stands, a content remains what it is. Consequently the essential difference between the two sorts of connections, which is phenomenally perfectly clear, that between subjective and objective phenomena—my tooth-ache or hunger and the table or the horse-race, for example—is obliterated, and it seems impossible ever to explain in a satisfactory manner the objective character of our perception.

The introspective theory contains one last difficulty. What are we to do if the introspections of different observers do not agree? There are many examples of this in psychology. The disputes about the imageless thought, about the so-called attributes of sensation, about the primary colours, are all cases in point. The traditional theory of psychological method cannot adduce a criterion to decide which description is the true one.

II. BEHAVIOURISM AS A METHOD OF ESCAPE FROM DIFFICULTIES OF INTROSPECTION.

Yet, in spite of all these objections, the generally accepted view about introspection may proffer one argument that seems unshakeable: how can we investigate mental facts, unless the observation of them leaves them unaltered? We shall keep this question in mind and return to it later on. Strongly opposed to the method of introspection are those of the Behaviourists, who describe behaviour as a sum of responses to environmental conditions, using such concepts as reflexes, simple or linked together, original or conditioned, instincts and habits. Certainly they use no sensations and no images in their statements of fact and in their explanations. But they are not so opposed to the introspectionists as they think they are; for their method of description is in one essential point just the same: it is as highly analytic as it can be. If we would observe behaviour we must, they say, observe it bit by bit, and keep down to facts that can be registered or measured. The ideal is always to know the direction and speed of the movements of each limb, the secretion of each gland, resulting from a fixed stimulation. The attainment of this goal would make prediction possible, but a prediction as unlike the foresight of a clever lawyer as the mathematical function describing the surface of the Venus of Milo by the three coordinates of space is to the living and meaningful beauty of the real thing. Behaviourism can in this sense never catch what it is after; real animal behaviour, true action, purposive movement directed towards an end. All it can do, if it remains true to its own principles, which fortunately it has never done, is to learn new special facts about the physiology of the muscles and the glands. And again this method demands a specific attitude of the observer; he is not to trust his first impression, which lets him see a clever dog searching for a hidden piece of food, or the slow deliberating glance of a chimpanzee wanting a banana out of his reach; he does not see rage or fear or joy, but only movements of limbs and secretions.

The ultimate position is similar to that of the introspectionist. Just as he never can explain the naïve objective meaningful perceptions on the basis of his subjective meaningless sensations, so the behaviourist would be badly off, were he compelled to account for our first impressions of animal behaviour. The two positions are not indeed identical. The attitude of the behaviourist observer cannot influence the facts observed; but it is similar to that of the introspectionist in so far as the actual observations which are made are in both cases dependent upon the general attitude which is adopted.

Thus, if introspectionism is wrong, behaviourism of this particular type must be wrong too. Yet behaviourism can offer a strong case: what else can we scientifically observe except analysed parts of behaviour, muscle contractions and gland secretions?

III. GESTALT-PSYCHOLOGIE AND THE DIFFICULTIES OF INTROSPECTION.

(a) *Abolition of Psychological Analysis.*

Let me now raise the question of how another psychology which renounces the assumptions so far discussed would proceed. Suppose we try to replace the analytic and substantial attitude by an integrative and functional one. We then begin with the organism in its environment, and study all its reactions. Among these we may reckon also those which are usually termed consciousness, I mean the fact that the physical world exists for the organism as its own surroundings—which we may refer to as its phenomenal world, though, of course, to the organism these surroundings appear as perfectly real surroundings—and the other fact that the organism also knows itself, feels hunger and pain, rage or lust. In experimental psychology the conscious side of behaviour plays a very important part; with this introspection is concerned; this we shall begin to discuss.

Any description I can give of my surroundings, of this room, the people in it, and so on, are facts for psychology to start from. Such descriptions have no connection with those called psychological by the strict introspectionist. Heaps or even groups of colour-sensations do not make a chair, or groups of sound-sensations a spoken phrase. Yet what are called colour- and sound-sensations do no doubt exist, that means they can be produced under specific conditions. In many cases, though not in all, it is possible to analyse an original whole into 'sensational elements,' or rather, to remain nearer the facts, to get meaningless sensations in the same situation that has already presented us with a meaningful, unitary whole. But if we try, for example, to perform an analysis of this kind upon a human face we shall inevitably fail.

The view which I have just described, then, has the task of bringing the facts of the unanalysed and the analysed phenomena together without using the hypothesis of substantial mental elements. This is achieved by adhering strictly to a functional point of view. The analysed datum, as well as the original one, must be considered as a reaction of the organism and the question to be asked is: "What is the reason for the occurrence of either of them?" It needs no new assumption to answer: "The conditions in the two cases must have been different, and inasmuch as the objective conditions are supposed to have been identical, the subjective ones must be supposed to have changed. You were looking at this room and were interested in it at the beginning, you were introspecting and interested in psychology afterwards. This change of attitude is clearly a change of subjective conditions, consequently we must not be surprised to find different reactions."

The discrimination between the conditions of a reaction, subjective and objective, and the reaction, again we may say subjective or objective, trivial as it may seem, is of the greatest methodological importance in psychology. I shall try to elucidate this by another example. The influence of memory on perception is, in current psychology, explained by 'assimilation.' A sensation arouses by way of association images of sensations that were aroused by former similar stimulations, and these images fuse with the newly-aroused sensation into one whole whose sensory and ideational components cannot be distinguished. This explanation is truly analytic, based on the sensation-elements, and substantial, as it uses substantial remnants of former elements which now reappear as mental entities. And in a way this theory, based on analysis, contradicts itself, maintaining that in the 'assimilated' percept analysis into constituent parts is impossible. Instead of proving its own assumptions, the solution proposed begs the question.

In lieu of this we say: the first reaction to a certain stimulation leaves the organism in an altered condition. Consequently, in a similar stimulus-situation the reaction of the organism will not be the same as the first, and will not be as it would have been without the previous occurrence of the first response; the perception will have features resulting from the change of subjective conditions in the organism. This is a description of fact without the many hypotheses implied in the older explanation, it is logically clearer, and, what is of chief importance, it has a much higher explanatory value.

This way of describing facts has very radical consequences. It leads to the abolition of psychological analysis in the sense hitherto generally understood. We are no longer allowed to maintain that a reaction which appears to be of a certain kind is really of a different kind, namely, one which is obtained under analytic conditions. The analytic method can, according to this way of thinking, only find out into what kind of reactions other reactions can be transformed by the change of attitude called psychological analysis. This knowledge may be an important contribution to our explanation of the original reaction, but it is no longer the sole method of explanation.

The original reactions are to be studied just as they are and not merely under the aspect of what they will become when analysis is applied to them. Only in this way shall we be able to find their proper laws.

The first consequence of this is that we cannot assume a point to point coordination between the objective and the phenomenal world. A line by itself and a line of the same colour, direction and length, but forming one side of an oblong or a triangle, are two different things.

(*b*) *Description and Function.*

Here one of the objections we have raised against introspection recurs. What if a person says he can see no difference between the two lines? We might argue that such a person, in comparing these lines, had adopted an analytic attitude and in consequence could not find the difference—and we should be justified in so arguing by the way in which such statements are usually expressed, namely, that 'as lines' they are identical. Yet, consistent though our standpoint would be, the argument would have no basis outside of itself. If we want to prove the reality of our descriptions we must go further. This we can do by a procedure which is the keystone of all experimental psychology, the test of all psychological descriptions: we must try to develop functional consequences from our descriptive terms and find out whether such consequences are confirmed by fact. I will take an example from Köhler's work on the chimpanzees. It refers to the long-standing dispute about primary colours. Köhler proved that in training an animal to react to one member of a pair of different greys, what the animal acquires is not an association between each shade of grey separately and the striving or avoiding reaction respectively, but it learns to react positively to a '*member of the pair*,' that is, to some thing characterized by its place or function in the larger whole embracing the two units. The proof rests in the fact that if the pair is changed in such a manner that one of the old members is retained in the new pair but occupies the relative place which the other one held in the first—being now, for instance, the lighter, when it was the darker —it then determines the reaction according to its place in the pair and not as an isolated stimulus whose effect is carried over from one combination to another. The pair of stimuli may be chosen from any uniform system, and in all cases the effect described is found to occur. This gives us a functional method by which we may decide whether a system is uniform or not, for we can investigate the range over which we can transpose without destroying the result of training. Thus it is just as easy to make these experiments, if instead of taking two greys we choose two tints between, say, red and yellow. So long as the transposition is kept within these bounds the result of the training persists. If, then, the description of the four primary colours is a true one, we should expect that transposition will no longer produce its normal effects if we pass these bounds. If, for example, we teach the animal to choose the yellower one of two yellowish-orange hues, will it, when confronted with a pair consisting of the yellower one of the first and a tint just a bit greenish react to the latter? It ought to do so, if yellow were a point of no particular significance, as those maintain who deny either that primary colours exist or that yellow is one of them. Some provisional experiments of Köhler tend to show that under such circumstances the reaction does not take place, and thus the analytic description of the four primary colours is confirmed in an objective and functional manner.

We have, then, a method of escape from one of the chief objections to introspection, yet we are still far from the solution of our problem. For now we have to face difficulties which the old theory was free from, namely, how can introspection still reveal the true facts, if by the very act of introspecting we alter the conditions for the occurrence of these facts and consequently alter the facts themselves? Also, how comes it to pass that analytic descriptions submit themselves to functional confirmation?

To find an answer to these questions we must formulate our new descriptions somewhat more closely. Instead of describing a given unity by enumerating the elements into which it can be analyzed we maintain that these wholes are what they are precisely in their specific character as wholes and that their parts—for they nearly always contain parts—are not pieces thrown together, but real organic members; that is to say that a number of the properties of these parts belong to them only so long as they are parts of this particular whole. Such properties may be lost, or be replaced by other properties, should the parts themselves become isolated wholes or parts of other wholes.

Thus the wholes, instead of being composed of their parts, really determine what the parts are to be. They have their own laws of being, by which are determined the kind of wholes that are stable, the kind that are prone to become transformed and the direction of such transformation. Four different general directions of transformation may be distinguished: (1) simplification, (2) structuration, (3) unification, (4) amplification and their opposites.

I will try briefly to explain what is meant by these. (1) *Simplification:* An example will help to make the meaning clear. After-images are as a rule simpler than the original perceptions both in outline and filling. Gaps are bridged, contours simplified. As Goethe pointed out, after-images of squares tend to become circles, a fact which once again reveals a close connection between description and function. Descriptively the circle is the simplest of figures and functionally it is the end toward which certain simplifying processes are tending. From the point of view of cognition it might seem as though simplification were a falling off, as in many cases a cognitive process brings about a higher degree of complication. Yet simplification has also a high cognitive value, as will easily appear.

(2) To understand what is meant by *structuration* we must consider its least degree: this would give us complete chaos. Though such absolute chaos does not in fact occur, yet it shows the direction of change which structuration implies. Compare, for instance, the impressions a painter, an average educated person, a schoolboy, and a dog have of a painting like da Vinci's 'Last Supper.' Here an increase of 'structuration' goes with an improvement of the reaction. Yet this also need not always be the case. A linen-draper might look at this picture and see the table cloth as its most striking part. There would then be a high degree of structuration, involving the texture and shading of the tissue, but this articulation would not be adequate to the whole. By a process of structuration a part may gain more weight than is warranted by the law of the whole. Such structuration may, under certain conditions, destroy the unity of the whole.

(3) Chaos is the least degree of structuration; the least degree of

unification is a mere summative connexion, a form of cohesion which may be called an *and-connexion*. In the painting of the 'Last Supper' an observer may see the Saviour *and* St John *and* St Peter *and* Judas *and* the table and so on. Unification also means progress. But it would be wrong to suppose that originally perceptive reactions were all such *and-connexions*. Probably there are very few, if any such, at the beginning. A face, for example, certainly does not appear to a baby as hair *and* eyes *and* nose and so on, but as something friendly or nasty. Consequently, in most cases the *and-connexion* is a product of evolution, of breaking up, and as such it may be of very high cognitive value.

(4) By *amplification* is meant the fact that perceptive and conceptual wholes tend to become more and more embracing. For the young child time consists of the present only—this not being a mere point without any extension, but covering the period of a certain activity, playing, eating, and so forth. A day is, for quite a long time, a limit for such a whole; all reference to the universe, stretching over space and time, present, past and future, is a product of development.

We can now return to the first of our problems: how can introspection reveal true facts, if it changes these facts? We have found that not all changes are equivalent. Some are in conformity with the inner laws of the wholes observed and some are opposed to them. If our attitude is such that it causes a change of the first kind, then the product of our introspection will be, though not the original datum itself, but a development of it. Metaphorically speaking, we started with the germ and have now the organism. If, on the other hand, our attitude involves a change of the second kind, we get a product which in its essential features may be different from the original datum. This is why it is wrong to attempt to maintain an analytical attitude at all costs as *the* method of psychology. In most cases such an attitude does not develop the germ, but destroys many of its original tendencies. The attitude which is legitimate has to be determined by the nature of each separate whole dealt with, and in this appears the art of introspection. Introspection, like every other kind of observation, is an art, and it is not an easy one.

The concept of sensation is the outcome of the analytic attitude. Sensations are real, but are not equivalent to the realities of our everyday phenomenal world. Being a reality, being a process producible under certain well established conditions, sensation is worthy of study. The investigation of sensation may even help us to understand better the laws of other and more natural phenomena, but it will not do so if the sensation is treated according to the teaching of traditional psychology, as a mental element.

But how can we know just what attitude is adequate in any given case? Phenomenal reactions are processes, and as such they possess their direction. It makes a difference to these processes whether they are allowed to develop along their own lines or forced into an anomalous course. From this it follows that the result of the adequate attitude should *feel* different from the others. For example, we hear the beginning of a melody. Soon we feel very well whether or not the melody is properly continued, even if we have never heard it before. Many musical effects are produced by a sudden apparent change in the trend of a phrase.

This helps to elucidate another point. In most cases the change produced by introspection, even if it is a good one, does not develop the germ completely. But it allows us to see more clearly the aim towards which development tends, and at the same time the distance between the aim and the original datum. The terms we use to describe the latter are usually borrowed from this aim and must be modified to show how far away from it the phenomenon has really been.

There is another answer still to our main question. It has already been shown that a fundamental principle of psychological method is to test a description by its functional consequences. Why then not simply say: any change is good that can be submitted to this test? To find an answer to this question we must consider the connexion between descriptive and functional facts.

(c) *The General Basis of Reliable Introspection.*

The second essential point of the new doctrine which I am advocating now becomes clear. The first, which helped us to solve the first problem, was the *meaningful* connexion between the parts of a whole, a connexion not based on mere coexistence, but on the very essence of the wholes involved. The second point is a psycho-*physical* one. *Those wholes are not particularities of mind.* If we begin to look for them, we find them everywhere in nature[1]. Consequently we are forced to assume such wholes in the nervous system, to consider psychophysical processes as such wholes, whenever we have reasons that suggest such a view. There are many such reasons. Experiment shows us numerous facts which are utterly inexplicable by any of the customary *psychological* theories. These facts point to a physiological interpretation which naturally must include the whole-properties of which I have been speaking. Moreover, the reactions of our mind which we called our phenomenal world are really but a part of our total behaviour, and even descriptively a phenomenon very often points beyond itself, either backwards or forwards. Let us consider a case in point: The successive comparison, say, of two noise-intensities. Here, the interval being neither too long nor too short, the second noise appears as either *louder* or *softer* than the first, rising or falling from the first, though the first is no longer in consciousness. The second noise then has a backward tendency, is bound up with a past which is no longer present in consciousness. We cannot, therefore, cut off conscious processes from processes not accompanied by consciousness. We must assume that conscious processes are part-processes of larger wholes and that, by pointing to other parts of the same whole, they give us evidence that the physiological process is just such a whole as the mental one.

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[1] Cf. particularly the work of Prof. W. Köhler.

To derive functional facts from descriptive concepts, therefore, means that we follow the conscious part of a larger process-whole beyond its conscious limits, and predict from the part we know the way in which it will be continued.

Functional and descriptive facts, then, belong closely together, and we can use the former to test the latter. This leads us back to the observation of outward behaviour, to the behaviouristic method. We can now understand why the observational results of the behaviourist are very meagre and tell us very little of the real activity of the animal observed. The activity-bits composing, in enormous quantity, the bulk of the behaviourist's studies, are presented as so many independent, isolated, elementary actions and not, as they ought to be, as part-processes of an embracing whole. The description, "This animal is running away from some danger," however insufficient it may be, is yet a thousand times more characteristic for the animal's behaviour than a formula giving us the movements of all its legs with their varying speeds, the curves of breath, pulse, and so forth.

But how do we come to make such assertions as that the animal is running away, or that it is angry, shy, stubborn, and the like? How can we ever confirm the statements, how can we ever *perceive* the behaviour? These were the pressing questions of radical behaviourism, and so long as we believe that analytic observation only can apprehend fact we can give no answer to such questions. By analytic observation such behaviour is not to be found. But, for mental facts at least, the analytic attitude is by no means the only or even a preferential method, and bodily and mental behaviour are parts of larger wholes. These wholes being real, they are not open to analytic observation. If an observer is to grasp their nature he must adopt a very different attitude, such as the attitude of the great knower of men, the wise old doctor, the clever diplomatist, or the 'intuitive' poet. The observability of *real* behaviour is then no longer so mysterious. Bodily and mental states, belonging to the same whole-process, will in many properties be very similar to each other. This holds chiefly for the dynamic aspect: rhythm, accentuation, rate, uniformity or change of direction, and so on. Such descriptions, as precise or vague, fast or slow, steady or abrupt, consistent or vagrant, apply as well to the phenomena of the person observed as to his bodily actions. It is an assumption that they refer *primarily* to mind and are only secondarily transferred to the bodies of living beings. If we drop this assumption, how shall we define the perceptive process in an observer of animal or human behaviour? He is confronted with the bodily movements of the observed organism; these being typical whole-processes, or parts of such, will produce in the observer perceptive whole-processes with similar properties. The pianist's finger movements on the key-board, to use an example of Köhler's, are the outlet of his mood, the transformation of it into a new material; the rhythm of the mood, for example, wanders from the mood through the fingers into the music and this is, by a sympathetic musical listener, grasped as a mood.

So in principle the possibility of such observation is established. Of course this sort of observation is very difficult, it is not everybody who will become a master in it, it is open to grave mistakes, as there is no guarantee that the whole-process aroused in the observer is the same as the process that produced the behaviour observed. And therefore it needs many safeguards and controls. These again must follow the principle applied so often. The chief point gained, however, is this: Psychology as the science of Behaviour is again allowed to describe real behaviour without exposing itself to the reproof of anthropomorphism. And such 'behaviourism' would have no need to banish real introspection as a method alien to all other sciences. For observation of behaviour and of phenomena (introspection) will be essentially alike and will yield essentially similar results.

AN INVESTIGATION OF GROUP INTELLIGENCE TESTS

BY R. R. DOBSON.

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I. *The Object of the Investigation.*

THE aims of the experiments described in this paper were the following: first, to determine if written tests of intelligence would select the brightest children in a School, and the brightest students in a University, using as a control both School (or University) records and teachers' estimates in combination; secondly, to determine by means of such tests whether there is a limit to the growth of intelligence and at what age that limit is reached; thirdly, to determine, if possible, which of the particular tests employed depend most on innate ability and which upon school environment and teaching.

II. *Description of the Experiments.*

(*a*) Subjects. Nearly seven hundred persons were tested. They belonged to three groups:

(1) Grammar School Pupils:
  (*a*) 222 boys in seven 1-year groups, ages 11–12 years, etc., to 18 years.
  (*b*) 37 girls in two 1-year groups, ages 16–17 years, 17–18 years.

(2) Elementary Teachers' Training College for Men:
  (*a*) 58 first-year students, average age 19 years 3 months.
  (*b*) 61 second-year students, average age 20 years 10 months.

(3) University Students:

  (1) Two-year students in training as teachers.

    (a) 21 men, average age 21 years 1 month.
    (b) 17 women, average age 20 years 3 months.

  (2) Students Reading for Degrees.

    (a) 8 men with Honours Degrees, average age 22 years 7½ months.
    (b) 18 women with Honours Degrees, average age 22 years 2 months.
    (c) 19 men, Degree not completed, average age 24 years.
    (d) 33 women, Degree not completed, average age 20 years 3 months.
    (e) 87 men irrespective of course, average age 22 years 6 months.
    (f) 97 women, irrespective of course, average age 23 years.

(4) University Lecturers and Professors, 6.

(b) The tests used were prepared by Dr Cyril Burt; and were practically identical with those used by him for the examination of several thousand Civil Service candidates. The booklet contained five tests, namely: (1) 50 Synonyms and Opposites, (2) 30 Analogies, (3) 25 Mixed Sentences, (4) 68 Completions, (5) 16 Reasoning Tests.

(c) The tests were all given by myself and were timed by a stop-watch. Three minutes each was allowed for Tests I, II and III, and 10 minutes each for Tests IV and V.

No writing was necessary, only underlining. One mark was given for each underlining correctly made; and the total (189 possible) was divided by 2 and treated as a percentage.

The tests were correlated with one another and with the pool of the teachers' estimates. The teachers' estimates were obtained by getting the ranking of the pupils by the various masters and correcting where necessary by the school records.

For University Students the ranking was made by the various Lecturers and Professors.

The correlations were made in all cases by the formula

$$r = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}.$$

III. *Results.*

The results are given in the following tables.

TABLE A.

	University students								
	University lecturers	Students irrespective of Course		4th year with Honours Degree		4th year Honours Degree Course not completed	Two-year students		Other than 4th year
		M	W	M	W	M	M	W	W
Number of Students	6	87	97	8	18	19	21	17	33
Average age	—	22,6	23	22,7½	22,2	24	21,1	20,3	20,3
Average:									
Test I	48·3	36·5	36 5	39	37	35·5	37	31·5	33·8
„ II	22·6	10·9	10 5	11·6	10·5	11·6	10	5	9
„ III	27·3	17·5	16	20·3	15	17·4	17	14	15·1
„ IV	60·5	53	51·4	57·6	56	53·2	51·5	42·2	46·5
„ V	16 6	12·3	12·3	13·1	12·2	13	10·5	11	12
Average totals	175·3	130·2	126·7	141·6	130·6	130	126	103·7	116·5
Coefficient of correlation	—	—	—	—	·58	·55	·40	·65	·58
Range of marks	167½–184	90–170	72½–174	123–159	106–150	104–160	101–156	72½–132	90–158

	Training College		Grammar School								
	2nd year	1st year	17–18	17–18	16–17	16–17	15–16	14–15	13–14	12–13	11–12
	M	M	B	G	B	G	B	B	B	B	B
Numbers of Students	61	58	9	13	18	24	24	31	48	45	47
Average age	20,10	19,3	17,4	17,9	16,5	16,5	15,4	14,5	13,5	12,5	11,6
Average:											
Test I	34	30·7	33·3	34·2	32·5	31·5	29 2	29·2	25·5	24·8	22
„ II	10·5	11·6	15	16	14·5	14·3	14·4	13·1	12·7	12·6	12
„ III	16·5	16	16·6	15·1	15·5	14·8	14·4	13·1	12·7	12·3	11
„ IV	49	48	55	53	51	46·7	45·2	44 8	41·5	36	34·5
„ V	10·6	10	11·5	10·5	11·7	8·7	9·3	9·3	7·6	5·4	4·7
Average totals	120 6	116·3	131·4	128·8	125·2	115·8	112·5	109 5	100	91·1	84·2
Coefficient of correlation	·55	·53	·78	·70	·70	·78	·64	·73	·66	·64	·62
Range of marks	82–162½	59½–165½	95–170½	108–152½	102–160	57–149	85–141	71–149	64–137	40–131	56½–132½

TABLE B. *Showing average Correlation of each Test with the other Tests.*

(*a*) Grammar School Boys.
(*b*) „ „ Girls.
(*c*) Training College Students.
(*d*) University Men.
(*e*) „ Women.

	Test I	Test II	Test III	Test IV	Test V
(*a*)	·41	·40	·38	·42	·34
(*b*)	·53	·55	·54	·52	·36
(*c*)	·41	·39	·38	·34	·26
(*d*)	·29	·34	·26	·30	·15
(*e*)	·30	·19	·31	·40	·30

TABLE C. *Agreement between Intelligence Rank and School Estimate.*

Boys, 11–12 years

Intelligence	School estimate
1	1
2	2
3	3
29	33
31	29
32	30
33	35
34	34
37	36
38	38
39	39
42	42
43	40
44	37

Girls, 17–18 years.

Intelligence	School estimate
1	1
3	3
5	5
6	6
8	8
11	11

Boys, 15–16 years.

1	1
2	2
3	3
23	23
24	24

Boys, 13–14 years

Intelligence	School estimate
1	3
2	5
5	1
6	6
12	13
16	16
17	17
22	20
25	26
28	27
30	35
42	46
43	40
44	41
45	42
46	43

TABLE D.

58 pairs of equal (total) scores of boys (chronological ages, 11–14) and University and Training College (men) (chronological ages, 18–24).
Each matched pair is of the same mental age as judged from equal intelligence score.
All boys' scores are above the average score of their age-group.
All men's scores are below the average score of their age-groups.

		University and Training College average scores.					
Range	Frequency	Test I	Test II	Test III	Test IV	Test V	Total
80–89 ...	7	19·3	5·7	10·7	39·8	8·6	84·1
90–99 ...	10	26·5	3·7	12·3	42·4	10·4	95·3
100–109 ...	20	29·2	6·3	13·5	45·8	9·7	104·5
110–119 ...	12	32·4	8·8	15·1	48·0	10·3	114·6
120–129 ...	9	36·7	11·2	17·1	48·6	11·0	124·6
Final average	58	29·4	7·1	13·8	45·4	10·0	105·6

		Average scores of Grammar School					
Range	Frequency	Test I	Test II	Test III	Test IV	Test V	Total
80– 89 ...	7	21·6	10·3	10·0	36·9	5·3	84·1
90– 99 ...	10	26·0	12·1	12·9	38·2	6·1	95·3
100–109 ...	20	27·5	15·3	13·0	41·3	7·5	104·6
110–119 ...	12	31·8	15·0	13·5	45·4	9·0	114·7
120–129 ...	9	32·0	18·0	16·6	48·3	9·6	124·5
Final average	58	28·1	14·5	13·3	42·2	7·6	105·7

	Test I	Test II	Test III	Test IV	Test V
Ratio of Average Scores $\frac{\text{Grammar School}}{\text{University Students}}$	·96	2·0	·96	·93	·76

IV. *Discussion.*

In Tests I, II, III, IV and V the marks rose gradually for each age group (Table A). In the first four tests the marks obtained at 17 were about 1½ times the marks obtained at ages 11–12.

In the fifth test, however, the marks at 17 were about 2½ times those obtained at 11–12 and this high proportion was slightly increased amongst University Students—a fact which appeared somewhat significant and which will be referred to in the discussion on Table D.

There seemed also to be a sex difference noticeable, particularly in Test V. Girls of 16–17 average in this test 8·7, compared with boys 11·7; and girls 17–18 get 10·5, compared with boys of 11·5.

The average mark rose steadily from 81 to about 130. With eight selected Honours Degree men, it rose as high as 139, and with the large number (87) of older Students the average University mark was high, but no higher than that of the older School boys.

At the Training College the average totals are the same as those of boys about 16 years old and considerably below the average marks of University Students.

The correlation of the various groups of boys and girls at school with the school estimate is everywhere about the same, and is fairly high, ranging from ·62 to ·78. The correlation for older students is not so high.

The average correlation of each test with the others (Table B) is not so high as between the sum of the tests and school record, but the first four tests correlate fairly constantly with one another. Test II is somewhat erratic, and Test V appears to correlate least with the others.

Some remarkable agreements are found for boys and girls between the rankings due to tests and to school estimates (Table C).

Some of the following facts are worth recording: K. L., aged 11 years, obtained the mark 132. It is interesting to note that this boy is working well with boys who this year will take the School Leaving Certificate.

M. A. P., aged 17, who obtained the mark of 166, obtained a National Scholarship at 15 years of age for Distinction in Mathematics in the Higher School Certificate Examination and has since obtained an Open Scholarship at Clare College, Cambridge. The top fourth-year University man obtained 1st Class Honours in Modern Studies—French and English.

Table D shows 58 pairs of equal total scores. There is an average difference of 6½ years between the ages of the Grammar School Boys and the Older Students—and between some of them there is a difference of 9 years. If the same total score represents the same mental age then the score in each of the five tests should be substantially the same. This is so for each of the Tests I, III and IV, but Tests II and V vary considerably. The younger pupils obtained twice as many marks in Test II, but only about three-quarters of the marks in Test V. It is just possible that Test II measures innate ability, and that Test V measures the effect of school environment, teaching and experience.

These results open up the question whether it is possible to rate the standard of intelligence by the total score, since it can be obtained from such widely different scores in the individual tests, and consequently only doubtful value could be placed upon a single figure expressing the general result in terms, for instance, of an Intelligence Quotient.

V. *Conclusions.*

(1) There is an increase in age performance in all tests up to the age of 17 years. There is little or no increase in score after that age. Judged by the total score it appears that, for the great number at any rate, intelligence, apart from experience, ceases to grow, except amongst people of exceptionally high ability. The University lecturers and pro-

fessors, for example, obtained unusually high marks, well above those obtained even by fourth-year Students with Honours Degrees.

(2) The object of the tests in selecting the brightest pupils seems to have been attained, The agreement for boys of known ability, judged by school records and teachers' estimates, and judged by the intelligence tests, was remarkable.

(3) The results of the Students at a Training College seem to bear out the fact that for the most part the best boys and girls from Secondary Schools do not enter the Elementary Teaching profession.

(4) The possibility of comparing the intelligence of young boys with older boys by means of total scores is at any rate questionable. The tests appear to measure somewhat different abilities at different ages. The power to reason seems to increase with age so that the "Old Dull" may get a better result in a reasoning test than the "Young Bright." On the other hand the "Analogies Test" seems particularly suitable for the selection of young pupils of good innate ability.

(*Manuscript received* 15 *March,* 1924.)

THE CONSTANCY OF THE INTELLIGENCE QUOTIENT—FURTHER RESULTS

By P. L. GRAY and R. E. MARSDEN.

1. *Object of the investigation* (p. 169).
2. *The tests used* (p. 169).
3. *The children tested* (p. 169).
4. *Change of I.Q.* (pp. 170–171).
5. *Children tested four times* (pp. 171–172).
6. *Consideration of special cases* (pp. 172–173).
7. *Conclusions* (p. 173).

1. *Object of the Investigation.*

We have continued the investigation which we began in 1920 and described in our paper on "The Constancy of the Intelligence Quotient[1]." In that paper we pointed out that if the I.Q. of a child is to have any diagnostic value, we must either know that it is constant within certain limits or determine the manner and magnitude of its variation. With the idea of finding what change, if any, takes place in the I.Q. with time, we have re-tested as many children as possible on the first, second and third anniversaries of the first testing. Thus the intervals between the tests are exactly one, two or three years.

2. *The Tests used.*

The tests used and the method of scoring were those described in our first paper. Though our later tests have confirmed our opinion that in one or two cases the first testing was not carried far enough in certain respects, yet we have included all our results without any selection whatever, This to some extent makes for the increase of the I.Q. changes.

3. *The Children Tested.*

Out of the 224 children originally tested we re-tested 100 at the end of one year, 63 at the end of the second year, and 57 at the end of the third year. A number of these, however, it was not possible to test on every occasion, some, indeed, being tested only twice. Thus in all 371 comparisons are available.

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[1] This *Journal*, XIII, pt. 3, Jan. 1923.

4. *Change of I.Q.*

In Table I are shown changes of I.Q. for all the children tested after intervals of one, two and three years. These have been grouped according to (1) the Natural Age at the earlier testing, (2) the I.Q.'s at the earlier testing, (3) the Mental Age at the earlier testing, (4) the Intervals between the testings, and (5) Sex.

TABLE I. *Showing I.Q. changes for children tested after intervals of one, two and three years, for children of different natural and mental ages, various degrees of brightness, and sex.*

	N.A. at earlier test				I Q Group					M.A.			Interval			Sex		
Changes in I Q.	3–6 years	6–9 years	9–12 years	Above 12 years	Above 110 I.Q	90–110 I.Q.	Below 90 I.Q	Above 120 I Q	Below 80 I.Q	4–8 years	8–11 years	Above 11 years	One year	Two years	Three years	Boys	Girls	Total No. of comparison
Greater than 20	3	—	1	—	2	2	—	—	—	2	2	—	2	1	1	2	2	4
16–20	1	2	6	1	4	4	2	1	—	3	8	4	8	5	2	4	6	10
11–15	—	6	9	7	7	14	1	3	—	5	7	10	10	5	7	11	11	22
6–10	2	20	28	7	10	32	15	4	2	18	24	15	33	19	5	30	27	57
1– 5	—	26	40	14	9	52	19	4	1	18	39	23	47	21	12	34	46	80
0	1	4	15	4	4	15	5	2	1	4	10	10	16	5	3	11	13	24
– 1– 5	5	37	47	14	23	61	19	8	—	22	53	28	64	26	13	50	53	103
– 6–10	2	25	10	7	10	30	4	7	—	11	20	13	19	17	8	17	27	44
–11–15	2	10	6	2	11	9	—	4	2	2	11	7	9	8	3	9	11	20
–16–20	2	3	—	—	4	1	—	3	—	3	2	—	1	2	2	1	4	5
Greater than –20	1	1	—	—	2	—	—	—	—	2	—	—	—	1	1	—	2	2
Totals ...	19	134	162	56	86	220	65	36	6	90	171	110	204	110	57	169	202	371
Median change	–1·6	–1·5	+1·2	+1·5	–2·3	+0·2	+2·0	–2·5	+2·5	+1·1	–0·6	+0·5	+0·2	–0·6	0·0	+0·4	–0·3	0·0
Semi-interquartile range ...	9·4	4·9	5·9	5·0	7·9	5·1	4·7	7·5	—	6·0	5·0	5·5	4·6	6·3	6·0	5·5	5·4	5·5

When the whole of the 371 comparisons are examined it is found that:

(1) The central tendency of change in terms of I.Q. is 0·0 points.

(2) The middle 50 per cent. of changes lie between the limits of 5·1 decrease and 6·0 increase.

(3) The probable error (semi-interquartile range) of a prediction based on the first test is 5·5 points.

(4) The correlation coefficient for the whole of the 371 comparisons is ·854 ± ·011. For the 100 comparisons we dealt with in our last paper the correlation coefficient was ·883 ± ·015.

Investigations similar to ours have been carried out in this country and in America, but the comparison of the results is made difficult by the fact that sometimes the results have been obtained by the use of two different scales for testing the same children: an earlier form of the Binet scale, and, later, the Stanford Revision. In addition, while we have limited our intervals to exactly yearly ones, the other investigations are based on intervals varying from a few days to several years. A summary of the American results is given in an article on "The Constancy of the I.Q." by Rugg and Colloton[1]. The central tendency of the changes ranges, for different investigators, from a decrease of 2·3 (Terman) to an increase of 1·6 (Rugg-Colloton).

The middle 50 per cent. of the changes are Terman −7·1 to + 3·8[2]; Poull − 3·3 to + 4·8; Rugg − 2·3 to + 5·6 and the Semi-Interquartile ranges are Terman 5·4; Garrison 4·66; Poull 4·6; Rugg 4·7.

The annual changes in Mental Ratio (I.Q.) for 72 children given by Burt[3] are from − 4·3 with 4 children age seven to − 2·0, with 15 children age eight. These average changes will compare with the average change of I.Q. given in Table II.

5. *Children tested four times.*

As the children usually leave school when they are fourteen years of age the number we can re-test grows smaller year by year, so that only 42 have been tested four times. In Table II are given the average I.Q.'s of these children grouped according to their I.Q.'s.

Table II. *Average I.Q.'s of 42 Children tested four times.*

		Average I.Q.			
I.Q group	No. of children	1st testing	2nd testing	3rd testing	4th testing
I.Q. less than 90	11	83·8	88·6	88·9	85·8
90–110	26	100	103·3	103·5	101·4
Above 110...	5	124·6	125·8	126·4	121
All	42	98·7	102·2	102·4	99·7
Change from previous year	42	—	+3·5	+0·2	−2·7

From this table it will be seen that all the groups show a similar rise at the second testing, in the third testing an approximation to the second, and then a drop in the fourth testing which brings the lowest group nearly back to the first I.Q. and the higher group drops below the first average I.Q. At first it was thought that the rise was due to practice but if that were the explanation there should be a further rise in the succeeding years and this has not taken place. The fluctuation may be partly due to the fact that the age-assignments of the tests for the American children are not entirely valid for English children. Though we have closely analysed our data and find that a few tests seem to be wrongly placed for English children, yet our total numbers are so small as to make it unwise to base any final age-assignments of the tests upon them. We have therefore accepted Terman's allocations throughout.

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[1] *Journal of Educational Psychology*, Sept. 1921.

[2] See footnote, this *Journal*, xiii, pt. 3, Jan. 1923, p. 317.

[3] *Mental and Scholastic Tests*, p. 155.

Among the tests that seem to require re-standardizing for English children is the Vocabulary Test. A fuller discussion of this test will be found in our paper on "Terman's Vocabulary Test[1]," in which we give the words in the order of difficulty as determined by testing over 400 children, and state that the scores assigned by Terman to the different mental ages are probably three words too high.

The correlation coefficients for these 42 children between the fourth test and the first, second and third, are given in Table III.

TABLE III. *Correlation between the testings for* 42 *children.*

Test		Correlation coefficient
4th and 1st	...	·843
4th and 2nd	...	·839
4th and 3rd	...	·834

The probable error in each case is approximately ± ·031.

It will be noticed that the correlation coefficients for these 42 children show little diminution with the lengthening of the interval between the tests. This appears to be in some way connected with the fact that the correlation coefficients are themselves smaller than those obtained from the whole of our comparisons. At the end of the first year we re-tested 100 children and obtained a correlation coefficient of ·883 ± ·015, but at the end of the third year, with 371 comparisons due to further re-testings, the correlation coefficient was ·854 ± ·011, a noticeable drop.

6. *Consideration of special cases.*

In our original paper we considered in detail all those children whose I.Q. showed a variation of ten or more points in the first two testings. Unfortunately we have been unable to follow up all the children dealt with, but it is interesting to see how far the later tests agree with the earlier ones. To save space the I.Q.'s alone will be given, a blank indicating that the child was not tested in the corresponding year.

R. D. 93, 123, 120, 115. The low I.Q. in the first testing was attributed to the fact that the memory for digits and syllables had not been fully tested. This proves to be a reasonable explanation as her later figures are much higher than the first.

G. F. 109, 120, 112, 107. This boy's memory for digits may not have been sufficiently investigated at the first testing, though we tested him up to the year in which he failed in all tests but one. In the other testings he shows a memory for digits well above the average of his age, though his I.Q. falls towards the normal. It is evident that the first I.Q. does him no injustice.

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[1] *Educational Research*, III, No. 2, March 1923, p. 3.

I. Q. 102, 113, 108, 108. A full account of this girl's performance was given in our earlier paper. It will be noticed that the last two I.Q.'s are approximately equal to the average of the first two.

M. C. 97, 108, —, 105. Half scores, which are not counted, would have brought the first figure up to 104 if included.

H. M. 119, 137, —, 118. This boy spreads his successes in the second test very widely; in the first and last test he shows a much more restricted range of performance.

J. A. 102, 112, —, 97. There seemed to be no reason for the large increase in I.Q. except the probability of exceptional growth. The last figure has fallen below the first.

Among the children we first tested in 1920 are 9 who have won Junior City Scholarships and 5 who have won scholarships to the Grammar School. The I.Q.'s of the City Scholarship holders were 101, 110, 114, 115, 119, 130, 132, 134, and those of the Grammar School 135, 140, 141, 152, 161. The boy with an I.Q. of 101 was said by his teacher to be an exceptionally steady worker. The examinations for scholarships were independent ones and the existence of the I.Q.'s unknown to the authorities.

7. *Conclusions.*

(*a*) There seems to be no very marked difference of variability or of median change in I.Q. Natural Age, I.Q., Mental Age, Interval between the tests, and Sex all appear to have no special influence on the change of I.Q.

(*b*) The probability of an I.Q. showing an increase or decrease of as much as 5·5 points is one in two; as much as 11, one in five; as much as 16·5, one in twenty, and for as much as 22 the chances are one in a hundred.

(*c*) Practice once a year appears to have little effect on the I.Q. as measured by the Stanford-Binet Tests. As the children are never told whether their answers are correct or not the same wrong answer is very frequently given in successive years. The Ball and Field test is the one which appears to be easiest to remember. We have known this to be used by itself as an intelligence test: this appears to be unwise apart from the fact that no one test can be a satisfactory measure of the intelligence.

(*Manuscript received* 26 *March*, 1924.)

THE RELEVANCE OF PSYCHO-ANALYSIS TO ART CRITICISM[1]

By A. M. BODKIN.

I. *Failure of analysts to consider art in the sense in which it is understood by art critics* (pp. 174–175).

II. *Reason for anticipating that psycho-analytic studies should be relevant to art criticism* (pp. 175–176).

III. *Relevance more apparent in regard to literary than pictorial art* (pp. 176–177).

IV. *Art critic's account of the emotional elements of design leads to consideration of differences of individual sensibility in regard to beauty of form* (pp. 177–179).

V. *Possibility of the study of dreams throwing light upon such differences* (pp. 179–180).

VI. *Psycho-analytic studies relevant when the art critic considers relation of artist's distinctive sensibility to his consciously acquired powers* (pp. 180–182).

VII. *Bearing of the above upon art education* (p. 182).

VIII. *Conclusion* (pp. 182–183).

I. *Failure of analysts to consider art in the sense in which it is understood by art critics.*

The aim of this paper is to consider the relevance of psycho-analysis to the understanding of art, especially pictorial art, as viewed by art critics themselves. In those references to art which we find occasionally in the writings of Freud and others of his school, no distinction seems to be made between art in the more exact sense, and the whole range of popular fiction, pictorial representation, and other forms of entertainment, which play their part in relation to the emotional and imaginative life of the community. It is to art only in this loose sense that we can apply the famous passage in Freud's lectures where art is described as a form of day dream whose appeal is general—a way of achieving, by elaboration of phantasy, the gratification of common instinctive needs. Or again, in the study that Pfister has made of the drawings of his 'expressionist' patient, he is concerned with the pictures only in relation to what they represent, and the patient's associations therewith. There is no indication that the drawings were works of art, or, if any of them were so, they are not considered with that question in view.

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[1] The substance of this paper was presented at a meeting of the aesthetics section of the British Psychological Society, May 16th, 1924, and was intended to carry further the discussion initiated by Mr Roger Fry's paper, entitled "What artists want from Psycho-analysis," read to the Society, November 17th, 1923.

If the psycho-analyst is to co-operate with the art critic in arriving at any deeper understanding of the nature of works of art, he must consider those characteristics of art which the critic finds to be distinctive. By many critics today the distinctive character of pictorial art is found not to reside at all in its aspect as representation, but rather to consist in its possession of a form directly significant to emotion. It may be that some critics have exaggerated the extent to which it is possible to distinguish between form and content, or to exclude associations with what is represented, in considering the act of aesthetic appreciation. Without disputing this point, one might perhaps define tentatively the nature of pictorial art, as an object of aesthetic criticism, by saying that what we have to understand and appreciate is the achievement of an organic unity significant to emotion, in which such sensuous elements as line, mass, colour, have value directly, through their relationships within the visible whole. The question, then, is whether any light can be thrown upon the nature of this achievement through study of psycho-analysis.

II. *Reason for anticipating that psycho-analytic studies should be relevant to art criticism.*

In speaking of psycho-analysis I am not confining the term to the work of Freud and his disciples. I am supposing rather that the critic, or student of art, includes in his survey the whole body of the writings of different schools of medical psychologists, regarding them as a storehouse of observations interpreted according to varying hypotheses, among which he may seek suggestion for dealing with his own problems. That these medical investigations possess a strong attraction for the art critic—even when he finds them disappointing—may, I think, be explained by the fact that they deal with products of phantasy in relation to the concrete whole of an individual life. Though the view-point and guiding interest of psycho-analyst and art critic is, in the main, so different, they have this in common. The medical psychologist relates the imaginative product—dream or waking phantasy, valueless as it may be—to the whole emotional and imaginative life from which it issued, because he is trying to understand this life with its inter-related mechanisms, as a working whole whose functioning he may assist. The art critic, because of the value he feels in the unique imaginative product studied, is led back to the unique imaginative life from which it issued, that he may understand and appreciate the work of art more fully. Both analyst and art critic have need of a concreteness and continuity in the study of the individual, which the more exact researches of the laboratory psychologist do not attain.

III. *Relevance more apparent in regard to literary than pictorial art.*

It is in the sphere of literary, rather than pictorial, art that the promise of help from psycho-analytic researches seems to have begun to find fulfilment. One may refer to the interesting study which Baudouin makes of the poet Verhaeren, in his book *Psycho-Analysis and Aesthetics*; though even here, a criticism might be applied similar to that made upon Pfister. The poems are analysed, as dreams are analysed, for the symbolism of their content, while formal values are unregarded. If there are, in the poems, passages where the sensuous medium becomes transfigured into a magic vehicle of emotional significance, the analysis does little toward the understanding and appreciation of this. One would expect the relevance of psycho-analysis to art criticism to appear most clearly where the form of the work of art studied is most completely an imaginative structure, whose elements are ideal rather than sensuous—"a thing carved in the stuff of thought." I should select the work done upon Greek drama, by such writers as Gilbert Murray, Jane Harrison, F. M. Cornford, as perhaps the best example of studies in which the results of literary and anthropological research meet those of psycho-analytic investigation and become mutually illuminating. The scholars show us the Greek tragic poet "working under the spell of a set traditional form[1]," or "ritual mould[2]." Into this form is "condensed" (to use the Freudian term) imagery connected with the seasonal cycle and with the cycle of the generations of men. In the working out of the theme of Hubris—blind arrogance and its penalty—we can trace the blending of emotional overtones pertaining to the fleeting glory of natural life at its height, and those belonging to the moral struggle of an individual hero. The saying of the philosopher Anaximander presents suggestively the blending of these overtones in the 'collective representation' that underlay the thought of the period and found expression in poetic creation: "Things perish into those things out of which they have their birth, according to that which is ordained, for they give reparation to one another, and pay the penalty of their injustice, according to the disposition of time[1]." In such studies of Greek drama as Mr F. M. Cornford's, we seem to find Jung's theory of the animation by the poet of the 'archetypal idea,' translated into detail in the course of independent researches, while at the same time the demand of the art critic is fulfilled that the form and unifying design of the work of art shall be considered, and shall be related to the emotional and imaginative life of the artist, and of the group within whose tradition he works.

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[1] Gilbert Murray's "Excursus on the Ritual Forms preserved in Greek Tragedy" in J. E. Harrison's *Themis*, 1912, p. 352.

[2] J. E. Harrison, *Ancient Art and Ritual*, 1913, p. 139.

IV. *Art critic's account of the emotional elements of design leads to consideration of differences of individual sensibility in regard to beauty of form.*

Is it possible to find anything similar to this in regard to pictorial art? Have art critics themselves attempted any account of pictorial art in relation to the imaginative life of artist or spectator; and if so, does their line of thought at any point make contact with the researches of psycho-analysis? In Roger Fry's *Essay in Aesthetics*, 1909[2], we find an account of the relation of the elements of design to the imaginative life. He enumerates the emotional elements of design as rhythm of line, mass, space, light and shade, colour, perhaps also inclination of planes, and speaks of these elements as "connected with essential conditions of our physical existence," appealing to the sensations accompanying muscular activity, and all the varied adaptations that we make to resisting bodies and to the force of gravity. He says "The graphic arts arouse emotions in us by playing upon what one may call the overtones of some of our primary physical needs." They so "arrange the sensuous presentment of objects that the emotional elements are elicited with an order and appropriateness altogether beyond what Nature herself provides." These quotations show that even a critic so rigorous as Mr Fry in dismissing associated ideas as irrelevant to aesthetic appreciation, recognises that some 'deep reminiscence' inevitably enriches the apprehension of visible form with the accumulated results of past experience.

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[1] Quoted by F. M. Cornford, *From Religion to Philosophy*, Cambridge, 1912, p. 8. The philosophic ideas presented in this book are applied to the study of drama in other works by Mr Cornford; see especially *The Origin of Attic Comedy*, London, 1914, p. 208.

[2] Reprinted in *Vision and Design*. Revised Edition, 1923.

In attempting to follow up this idea of the emotional elements of design, I should like to make some reference to my own experience in relation to the problem; since it is the personal approach of the writer to the question he deals with, that inevitably determines the emphasis of the thought, and his contribution is often more readily evaluated if this line of approach is exhibited.

My own interest in Aesthetics may be said to have begun with the reading, some years ago, of Vernon Lee's discussion of empathy[1]. From her examination of her own apprehension of beauty, and comparison of the accounts given by her correspondents, an idea emerged which seemed to me to call for further development—that of the relation of the individual's apprehension of beauty to the unconscious, or half-conscious, habits of his emotional life. In the case of Vernon Lee, and others, it appeared that motor factors, ideal rather than sensational—the revived, accumulated results of motor experience—formed an important element, discernible by introspection, in the aesthetic appreciation of visible shapes. I found Vernon Lee's conclusions illuminating in regard to my own experience in the enjoyment of landscape. I had wondered at the difference it made when one found the right point of view, where lines and masses seemed to fall into harmony, where, looking again and again, one could enter into the pattern of the landscape and feel it become animated. One seemed to see lines that moved rejoicing to a climax, and shapes that stood triumphant, as the body stands poised at some movement-climax of a dance. The question presented itself as to how elements of a visible whole should come to be felt as energies acting upon one another, and the answer seemed to me to lie, as Vernon Lee maintains, in "the attribution of the modes of our dynamic experience" to the visible forms. Students of the laborious analyses of space perception that figured so largely in the older psychological text-books, were familiar with the idea of memory-traces of movement, fused with present sensation, determining practical awareness of the configuration of objects; but less attention has been given to the effect of such fused memory-traces in influencing aesthetic vision. According to the course of the individual's aesthetic development, and the modes of interweaving of contemplative and active pleasures in the growth of his mind, the psycho-physical dispositions that determine his apprehension of beauty will come to be differently organised; though all this fashioning must be in the main unconscious, too intimately a part of the individual's idiosyncrasy to be recognised in language and compared by him with the experience of others[1].

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[1] Vernon Lee and Anstruther Thomson, *Beauty and Ugliness*, London, 1912. Vernon Lee, *The Beautiful*, Cambridge, 1913.

V. *Possibility of the study of dreams throwing light upon such differences.*

The use made by medical psychologists of dreams for gathering hints as to the obscure course of the emotional life, suggests that where any experience of beauty enters into a dream, some indication might be obtained of the nature of the individual's aesthetic sensibility, apart from the influence of consciously acquired prejudices or opinions. In the recent medical literature of dreams there is little to throw light upon this question, though, from the side of art criticism, comes an occasional reference to dream images, conveying the recognition that the dream has so much at least in common with the artist's vision, that it gives you the object simplified, modified into some intimate relation with your own feeling and mode of apprehension. An example of such a reference would be Laurence Binyon's comment on the waves in a Chinese screen-painting: "We might in dreams see waves such as these, divested of all accident of appearance, in their naked impetus of movement and recoil!"[2]

In my own memories of dream landscapes felt as beautiful[3], what specially strikes me is the way in which the lines of the forms seen are constantly realised in terms of movement, as though the memory-traces which remain fused in waking perception, escape from such synthesis in the dream. For example, I see the shapes of poplars against a night sky, and in the dream follow in an experience of bodily flight the upward soaring lines, or I see a wall of mountains, and move over them with movements that are like the upward and downward sweeping lines that one sees with such vivid realisation from some precipitous ledge when mountain climbing. I find the suggestion of a similar relation between imagined movement and vision in Mrs Arnold-Foster's *Studies in Dreams*, where, after telling of her recurrent and detailed experiences of flight in dreams, she remarks, of the dream-buildings which "seemed to the dreamer at least to be new and noble in design," that their lofty corridors and great staircases and doorways seem constructed rather to fly than to walk in. Whatever account analysts may give of the determinants of flying, and similar movement experiences, in dreams, they seem to add to the memories available in waking life, a factor of a peculiarly intimate nature, as an overtone in the enjoyment of certain kinds of visible form.

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[1] These fused associations need not, of course, be predominantly motor. The question here considered seems connected with the problem of aesthetic adaptation discussed by Mr Bullough, "Recent work in experimental Aesthetics," this *Journal*, XII, especially where he speaks of "a whole system of dispositions and tendencies orientated towards aesthetic experience," p. 98.

[2] Laurence Binyon, *The Flight of the Dragon*, London, 1911, p. 21.

[3] I am assuming that an experience of beauty is possible in a dream, yet from my own dreams I do not feel sure of this. It is perhaps only when the emotional dream image is 'distanced' in waking recall that an aesthetic experience occurs. However, the dream may still give indication of the sensibility that has moulded the material for this experience.

VI. *Psycho-analytic studies relevant when the art critic considers relation of artist's distinctive sensibility to his consciously acquired powers.*

So far, I have spoken of the merely appreciative attitude toward beauty, and of the dispositions active within the attitude, which have been fashioned in the main unwittingly. In considering the creative vision of the artist, we have to take account of the effect of his mastery of technique. I may refer here to Mr Bullough's discussion of the "images de traduction," or schematic images developed by the artist "in the course of practice and study by the accumulation of images framed in terms of his medium[1]." The artist's intuition of beauty, and the whole imaginative life of which it is part, must be profoundly penetrated by these ideas corresponding to his acquired powers of realising desired effects within his medium. It is, I suppose, the thought of this technical mastery that makes Roger Fry say that "Art as created by the artist is in violent revolt against the instinctive life, since it is an expression of the reflective and fully conscious life[2]." One might demur at this, thinking of all the unrecognised factors that must influence the artist's vision, yet the sharp realisation of means to ends, involved in art as a technical achievement, is pre-eminently the concern of consciousness.

It is here especially, in considering the relation between the unconscious emotional factors of the artist's sensibility and his deliberately acquired powers, that the art critic may find suggestion in psychoanalytic studies. At this point, the form of the problem of art critic and of medical psychologist seems the same, the one concerned with a specialised power, the other with a more general adequacy to life's demands. Thus, the analyst studies the conflict in the mind of his patient between natural impulse and the requirements of society, as recognised in the patient's habits and ideals. He may find the patient weak because the fundamental energies of his emotional nature remain outside the habits he has established under pressure of his social environment. The aim is to liberate these energies that they may find expression in more harmonious conscious activities and relationships. Similarly, the art critic who studies the whole output of an artist, with the desire to understand and appreciate his unique gift or virtue, will find parts or phases of his work that seem weak, mannered, imitative perhaps, in which his real power has not found expression. Only in certain of his achievements does he rise to the full height of his individual gift. The triumph seems to depend upon the achievement of harmony between the instrumental powers that he, in conjunction with the influences of his time, has fashioned for himself, and the promptings of the spontaneous sensibility that nature and the course of his emotional life have fashioned for him.

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[1] E. Bullough, "Mind and Medium in Art," this *Journal*, XI, p. 38.

[2] *Vision and Design*, p. 73.

In two recent books by J. Meier-Graefe—Studies of Van Gogh and of Degas—examples may, I think, be found of the sort of psychological study of an artist's mind that I am now considering. The author seeks to trace in his subjects the course of the artistic impulse struggling for fulfilment against whatever obstruction of temperament or environmental influence stood in its way. I will take a single example from Meier-Graefe's study of Degas, to illustrate the application to the study of an artist of such psychological considerations as pertain also to psycho-analysis. In discussing the influence of Ingres upon Degas[1], Meier-Graefe suggests that the system which Ingres practised with his pupils was suffocating to the individuality of many of them, because it was a system of conscious regulation developed in intimate relation to the master's own powerful temperamental impulse. Where such Romanticist impulse to expression was not present, as in Degas he thinks it was not, the artist might be oppressed rather than aided by such a system. It is partly, I think, through psycho-analytic studies that we have become more sensitive to this kind of relationship between a man's conscious philosophy of life, or system of rules for a particular activity, and the unrecognised urgencies of his own nature that make those rules important to him, either for good or ill. In a later passage[2], where Meier-Graefe is describing the almost instant triumph of Degas when he gave up oil painting to work in pastel, he notes that Degas had

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[1] J. Meier-Graefe, *Degas*, translated by J. Holroyd-Reece, 1923, p. 26.

[2] *Op. cit.* p. 76.

"combined with his painting an unattainable ambition to paint like Ingres." There is a suggestion of the question how far that ambition carried with it inhibiting influences, the liberation from which was an important factor contributory to the success which attended the change of medium.

VII. *Bearing of the above upon art education.*

The bearing of these ideas upon problems of art education is obvious, though it cannot be developed here. The question of the liberation of the individual's distinctive gift should be the central one for the teacher of art, as it is central for the art critic who traces the evolution of the artist's power. Roger Fry, in discussing the question whether art can be directly taught[1], observes that since it is a matter of "the full development of a sensibility which is peculiar to each individual," it is questionable whether teaching "the results of other sensibilities" can be helpful; and suggests that the process known as art teaching often proves destructive of whatever promise of originality was present in the child's work. The experiments of analysts, upon themselves and with their patients, in the so-called "drawing from the unconscious[2]," seem to show that people unconscious of any power to draw may produce drawings of considerable vigour of design, if they can free themselves from inhibiting influences present in their ordinary consciousness. The inhibiting influence may well be considerable that operates when a teacher imposes his methods and example, however excellent in themselves, upon a pupil whose natural sensibility and spontaneous impulse are very different from his own. One may recall the outburst of Blake against those great artists, Rubens, Correggio, Titian, who to him were "cruel demons," "snatching away" his vision, "weakening and darkening" the power of imagination.

VIII. *Conclusion.*

In summing up my answer to the question put forward in this paper, I would say that the bearing of psycho-analytic investigations upon art criticism must be mainly indirect, operating by way of suggestion upon the mind of the critic pursuing his own problems in his own way. A literary critic[3] has spoken of the spread of ideas that subtly modify a man's whole outlook: "They make a sensitive surface where there was nothing but a blind integument: they create dim centres of vision for what was before total darkness." It is in some such way as this that ideas that underlie the work of analysts using different hypotheses, and putting forward various and precarious conclusions, may operate upon the minds of critics and investigators in different spheres, quickening them to new discernments. Such ideas may be helpful in relation to the criticism of pictorial art, I would suggest, especially, as regards the study of: (*a*) the subconscious, emotional and imaginative life of individuals, determining their distinctive aesthetic sensibility; (*b*) the relation of this subconscious life and distinctive sensibility to the conscious acquisition and use of technique.

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[1] In *The Nation and Athenaeum*, February 23rd, 1924, p. 732.

[2] See *e.g.* the drawings reproduced in Dr Hinkle's book, *The Recreating of the Individual*, New York, 1923.

[3] Article on Herman Melville, *Times Literary Supplement*, July 26th, 1923.

THE DIDACTIC VALUE OF LANTERN SLIDES AND FILMS

By G. RÉVÉSZ and J. F. HAZEWINKEL.

(*From the Psychological and Paedagogical Laboratory, Amsterdam.*)

I. *The Problem* (pp. 184–186).
II. *Methods of Investigation* (pp. 186–188).
III. *General results of the first Experiments* (pp. 188–192).
IV. *Experiment with an Interrupted Film* (pp. 192–193).
V. *Retentiveness after a considerable lapse of time* (p. 193).
VI. *Results after an oral Explanation* (pp. 193–194)
VII. *Analysis of the descriptions* (pp. 194–196).
VIII. *Summary* (pp. 196–197).

I. *The Problem.*

The widely spread popularity of projections on the screen, their significance in broadening mental life and, especially, in prompting the psychical development of the masses and in stimulating scientific research have naturally paved the way for their adoption for teaching purposes. Many writers, in discussing the cinematograph, have stated that it can be of very great service in schools. Even those who from an ethical and aesthetical point of view hold that the cinematograph has a baneful influence are only antagonistic to the *programmes* of the picture-theatre; they recognise the social, educational and scientific importance of the cinematograph and warmly advocate the spread of the film in the field of science and instruction.

No reasonable objection can be raised against the cinematograph as a means of spreading knowledge, or as an auxiliary in scientific research and instruction. About its usefulness in these fields all are agreed, but there is no consensus of opinion, when the merits of the film are weighed against those of the camera, and when the question is put whether the film is in every way to be preferred to the lantern-slide and can entirely replace it. This question can not be answered off-hand, but only after a thorough experimental investigation.

This then is the problem which we set ourselves to solve, what are the advantages of the cinematograph, as compared with those of the optical lantern? Obviously it is possible to establish and estimate many serviceable applications of either without exact experiment; but this does not bring the problem any nearer to its solution, so long as the respective results are not comparable. Some results will speak for the one method, some for the other; but a practical conclusion can be arrived at only by a careful and continued comparison of the effects which both methods of display have upon the understanding of the pupil.

When it is our object to represent nature in all its aspects and details, the cinematograph is likely to render important services. One may feel inclined to say that it should be employed whenever it is intended to create a vivid impression, a representation true to nature attended by a great deal of change. But from the fact that moving pictures make strong and immediately perceptible impressions on us it does not follow that these impressions will last. On the contrary, owing to the rapid succession of pictures in the film, the power of retaining observations in memory is apt to suffer. Indeed we observe that quickly changing film pictures show a tendency to obliterate each other. It stands to reason that children show a distinct liking for cinematograph pictures, follow them with close attention and take more interest in the activities of men and animals than in stationary objects. The child is struck by the dramatic realities of the cinematograph, and does not care much about the permanent information that might accrue. Its great attractiveness, however, proves nothing either for or against its didactic value. The never flagging interest, the exuberant enthusiasm which we invariably observe in children when a film is shown to them, may be easily understood from a psychological point of view. But this does not prove that films are of equal value when the permanent improvement of the mind is considered.

The above observations have already been the occasion of some comment on lantern-slides. It is of particular importance to consider their value as compared to that of the film. By means of lantern-slides we are enabled to show an unlimited number of pictures in succession, and some few in juxtaposition. They offer facilities for observing and describing all the details separately and correctly, drawing certain particulars into prominence and leaving others in the background. All this is impossible with the quickly rotating film. It is not only that the stationary pictures of the magic lantern are conducive to correct observation of essential features, but also the mere looking at them is not without valuable results. They enable the operator to select those things to which he wishes to draw special attention and to eliminate any which might interfere with the aim he has in view. These advantages are wanting in the film, which attracts the observer's attention, willy-nilly, to the moving and changing elements, however interesting surrounding details may be. One must have a particular mental predisposition to single out for close observation relatively motionless figures, and even in such cases one can rarely succeed in repressing the moving element in the picture in favour of the motionless element in one's thoughts. It has been remarked that the close regard of lantern-slides demands much greater mental exertion than is required by the film. It is asserted that the comparative passiveness on the part of the observers which the film permits is responsible for its popularity. The film runs with a rapidity which baffles our attempts at assimilating important elements. All our interest is mechanically centred on one essential, *i.e.* the action, which is of importance for the concatenation of the different pictures. This interest may be termed a 'gliding interest'; it calls for less mental exertion, less concentrated attention. The so-called indefatigability of the film observer is, accordingly, chiefly due to the rapid change of the pictures and to the fact that the length of the film cannot come into line with his powers of observation, so much so that he seldom has an opportunity of concentrating his attention wholly on it. All this he knows by past experience. He will not take the trouble to give close attention to the pictures separately, but simply allows himself to be carried away by the plot. That one is able to reproduce the impressions received at a film show may be ascribed to its remarkable resemblance to nature and to its strong emotional force, in consequence of the natural and easy way in which the different scenes are connected in the mind—in other words to the plot.

II. *Methods of Investigation.*

Our test of the relative educational efficacy of the film and lantern-slide will be the way the pictures are retained in memory. This will decide which method is better adapted to produce lasting impressions on the mind. To answer this question we have first to ascertain what the child knows, in other words what are the facts of which it has accurate conceptions. The basis of our estimation was a comparison of the facts actually observed by the children. The first group of facts we determined by adding together all the facts, differing qualitatively, which had been accurately observed. We believe that it is better to take this number than that which might have been obtained by the operator himself acting as the subject. As facts worth observing we selected such as were essential to the story and made for a right understanding of the whole, leaving all facts that were of little or no importance on one side. To assign a numerical quantity to the facts retained by the children, we used the formula a/p, in which a stands for the number of facts actually observed, p for the number of facts which might have been observed. We also collected data as to the reliability of the results, *i.e.* a comparison of actual facts and erroneous impressions. It is, indeed, of importance to ascertain not only which mode of projection yields the largest number of accurately observed facts, but also the method which secures the greater accuracy.

We experimented with about eighty boys and girls, pupils in the Amsterdam Lyceum. The pupils of the two lowest forms and of the two second forms were the first to be experimented on. The average age of the children was thirteen and fourteen. Afterwards we tried the two fourth forms of the Grammar School, on an average sixteen years of age, and the three third forms, average age fifteen. The children belong, for the greater part, to the well-to-do classes.

Our topics were taken from geography. They included the life of the Bataks in Sumatra, tea growing, rubber culture, the environments of Buitenzorg and rice-culture in Java. In choosing our films and lantern-slides we were careful to select subjects rich in action, especially such as gave scope for observing the greatest possible variety of striking activities. The films used had not been shown before and the subjects in question had not as yet been dealt with in the geography lessons in the lower forms. About two years before they had been discussed in the higher forms without any apparatus whatever. The boys of the lower forms had no knowledge of the subjects shown and this was the main reason why they were selected for our experiments.

A technical difficulty arose from the trouble of finding films coinciding with series of lantern-slides. Fortunately the Colonial Institute of Amsterdam was ready to put its collection of films and lantern-slides at our disposal. By the aid of these we were enabled in our experiments to employ series of slides which corresponded to parts of some films, the films and photographs having been taken at the same time. In order to secure a reliable basis for comparison of the two methods of projection we endeavoured to make the time allotted for each as uniform as possible. The length of the films ranged between 220 and 300 metres, the number of slides varied between thirteen and sixteen, and the time of each method of representation averaged from 50 to 60 seconds. In order to equalise as much as possible the various conditions of experiment and to eliminate all possible errors which might render comparison unnecessarily involved, we repeated each representation with both parallel-forms of the first and the second year. We did not anticipate that our experiments with pupils of parallel-forms would yield the same results. But in point of fact the results proved to be practically identical.

The way we set about our tasks was as follows. One day we presented a given film to the first form, 1 (*a*); on the same day we showed the corresponding series of lantern-slides to the parallel form, 1 (*b*). A week later we set each form to write an essay on what they had seen. Three weeks after we repeated the experiment with the two forms using another film and another series of lantern-slides, and reversed the order of the presentation. We proceeded on the same lines with the parallel-forms of the second year. We stopped experimenting when in all eight tests had been made.

We deviated from the usual procedure by getting the pupils to write down what they had observed after the lapse of a week instead of asking them at once. This we did because our aim was to obtain evidence as to the relative hold each method of representation had on the memory. The essays were, therefore, written when the impressions on the mind had lost some of their original freshness and the ability to reproduce the facts had become relatively permanent.

In describing what they had observed the pupils were entirely left to their own devices. They could take their own time. Although the data obtained from answers to set questions are more numerous than those which may be expected from *ad libitum* descriptions, we preferred the latter as it left the pupils more freedom of expression.

III. *General results of the first Experiments.*

In the following table the data of the eight experiments are collected in the first and second forms, as shown by 164 essays, 82 dealing with the impressions derived from the film, and 82 with those derived from lantern-slides.

TABLE I

		Film		Lantern-slides		F.	L.	Proportion
		ac.	p.	ac.	p.	%	%	F.: L.
Facts of primary importance	I	385	834	517	834	46	62	1 : 1·35
„ „ „	II	441	1078	523	854	41	61	1 : 1·49
„ „ „	III	142	506	254	506	28	50	1 : 1·79
„ secondary „	I	179	656	208	656	27	32	1 : 1·16
„ „ „	II	296	1248	318	1032	24	31	1 : 1·30
All facts taken together I	...	564	1490	725	1490	38	49	1 : 1·29
„ „ „ II	...	737	2326	841	1886	31	45	1 : 1·41

ac. = accurate p. = possible F. = Film L. = Lantern-slides

It stands to reason that not all the facts and actions in the lantern-slides were to be found in the films, and *vice versa*. To insure a reliable comparison of the facts remembered by both modes of procedure, we have, therefore, first of all compared the things which were shown by the film as well as by the lantern-slides.

For the purpose of increasing the number of data, we have grouped under II not only the facts which appeared on the film as well as in the slides but also those which could only be found on either the film or the slides.

In collecting these data it struck us that many of the facts observed were so obvious that they could not very well have escaped the pupils' notice: they were absolutely necessary for the right understanding of the process represented. Thus in their description of the rubber culture none of the pupils failed to mention the incisions in the trees and the receptacles in which the juice is gathered. Such obvious facts or actions have been omitted from those marked primary and mentioned under III. By eliminating the equalising factor of those obvious facts or actions we hoped to throw into relief the relative advantages and disadvantages of the two methods of projection.

The columns marked 1 and 2 respectively represent the results of the first four film experiments: the first column gives the aggregate of objects and actions observed, the second column those which might have been observed. [On page 188 these were denoted by ac. (accurately observed) and p. (possible).] In the same way the third and fourth columns respectively denote the results of the optical lantern experiments. The fifth and sixth columns respectively give the percentage ratio of *a* to *p*. Lastly the seventh column gives the ratio between the film and optical lantern projection percentages.

Even a superficial view of Table I clearly reveals the fact that the results obtained by the lantern-slides are more favourable than those obtained by the film. The most favourable ratio is obtained by the 'primary facts' when the 'obvious facts' are deducted. It is obvious that these remaining facts are of the greatest importance, because they represent things which must be seen and cannot very well be described. The ratio 1: 1·79 in favour of the optical lantern apparatus would, indeed, suffice, even if the other ratios were slightly unfavourable, to prove it more suitable for educational purposes than the film.

It is especially in revealing the main facts of the scene that the optical lantern appears to be superior to the film, but our table also brings out its distinct superiority with regard to the minor facts. Even a comparison of the total of remembered facts, 841 as against 737, the optical lantern shows itself to be the better instrument, notwithstanding the larger total of observable facts in the case of the film.

We have thought it advisable to add some diagrams to the above table because the superiority of the lantern-slide might be thought to have been due to some particularly gifted pupils. In diagrams I–IV the vertical line axis represents the whole of the facts observable in the two comparable experiments. Along the horizontal axis of diagram I each square indicates one pupil in each of the two experiments. The best results are placed to the left, the worst to the right. The dotted horizontal line drawn across the diagram stands for 50 per cent. of the observable facts. Only the main facts (indicated by 1 in the Table I) have been taken into account. The average is to the advantage of the optical lantern method, the ratio in the table being 1 : 1·35.

In considering diagram I, which represents the results of the first experiment in the lowest forms, we may notice that 11 of 20 pupils remembered more than 50 per cent. of the facts by aid of the lantern-slides, 4 pupils exactly 50 per cent., while 5 pupils had remembered less. As regards the film there was only one pupil who remembered more than 50 per cent.; 19 remembered less, while 9 were below the figure obtained by pupils who had remembered least of the lantern-slides. The diagram shows that each boy, taken separately, observed the facts shown by the optical lantern more accurately than those presented by the film. It is not to be supposed that unusual intelligence of the pupils in this particular form is responsible for the better results obtained with lantern-slides. For a second experiment was conducted with the same pupils, in which another film and another set of lantern-slides were used, but the parallel form which had seen the film in the first experiment was now shown the optical lantern, and the other parallel form was given the film. Similar results were obtained (see diagram II).

When we examine diagram IV, which gives the results of our first experiments with the second form, we notice that the two lines are closer together; 4 pupils observed an equal number of facts in each representation, none has, however, shown better results with the film. In the optical lantern representation 10 pupils exceeded the 50 per cent., 2 just attained the 50 per cent., 8 fell below it. The film yielded the following results: 6 exceeded the 50 per cent., 3 just attained it. Also the control rendered by diagram III confirmed our conclusion that the higher forms, *i.e.* with older pupils, the difference between the film and the optical lantern results tends to become smaller.

We have also tested the pupils of the fourth form, 13 of each division, but the approaching holidays prevented us from checking the results obtained by a controlling experiment. The percentages were respectively 47 per cent. and 54 per cent., the ratio being accordingly 1 : 1·15. This experiment appears to show the superiority of the optical lantern, although in a less degree than in the experiments with the lower forms.

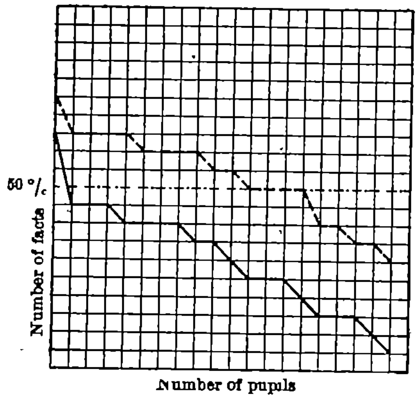

Diagram I

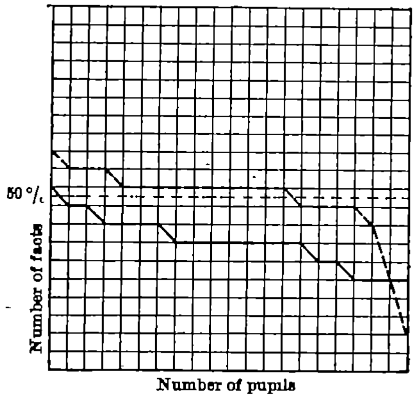

Diagram II

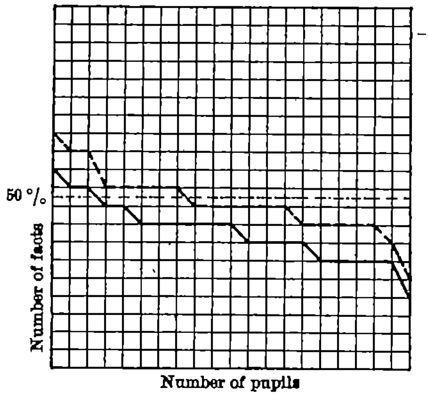

Diagram III

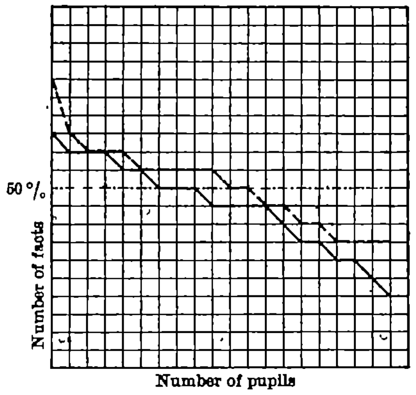

Diagram IV

---- Results by the lantern-slides. —— Results by the film.

The experiments need to be performed with still older and younger persons. But we have only been able to carry them out with children below the age of twelve, and have found that without oral explanations the film is of no didactic value whatsoever to them, while the optical lantern also loses much of its efficiency, as might have been expected.

It is still an open question with us what would be the results of either method of projection with boys and girls older than seventeen or eighteen, and also whether the film or the optical lantern is more instructive with persons of mature age.

IV. *Experiment with an interrupted film.*

As is well known, the mechanism of the film is such that the operator can stop it at any moment. It might, therefore, be urged that the interrupted film has the advantages of the continuous film and the lantern-slides combined. Although no technical difficulties stand in the way of changing the continuous film into an interrupted one, experience shows that as a means of instruction the latter does not yield the results anticipated.

We experimented on the third form, which happens to contain three divisions, so that we were enabled to compare the results of the three methods, viz. interrupted film, continuous film and lantern-slides. As we were not in a position to perform the ordinary controlling experiment, we examined the second form also as to the two film-methods. We selected the rice-culture in Java as our subject, which was excellently adapted for our purpose. This film consisted of about fifteen parts, strikingly independent of each other; each part was like a lantern-slide because only the persons moved and the background remained unchanged throughout. Moreover, all the parts were of equal importance, and there were no facts of minor importance to distract the attention. In a word it might be pronounced an ideal school-film. Fortunately each part was matched by a corresponding lantern-slide, mostly with the same persons in the foreground. The time allotted in the third form to the lantern-slides was the same as that allotted to the continuous film: the time allowed to the interrupted film was 30 seconds at regular intervals, *i.e.* 6 minutes longer. As this difference might be prejudicial to the accuracy of our conclusions, we made the time of the two methods the same length in the second form.

Tables II and III give the results obtained from the third and second forms respectively.

TABLE II. *Third form.*

	Interrupted film in %	Continuous film in %	Lantern-slides in %	Proportion		
				I.	C.	L.S.
Facts of primary importance I	32	32	34	1	1	1·06
” ” ” III	12·5	12·25	17	1	1	1·35

TABLE III. *Second form.*

	Interrupted film in %	Lantern-slides in %	Proportion	
			I.	L.S.
Facts of primary importance I	29	34	1	1·13
” ” ” III	10	16	1	1·65

Inasmuch as the distance between the curves in the diagrams I–IV lessens as the age increases, we may assume that the last experiment would have yielded no worse results in the third form than in the second, although no controlling experiment was made in the former. It seems safe to say that the interruption of the film does not materially increase the observing power of the pupils. The difference, which is only 0·4 per cent., may be considered negligible and does not justify any inference as to the advantage of the interrupted film.

The ratio of the results obtained by the film (whether interrupted or continuous) to those obtained by the lantern-slides, namely, 1 : 1·35 (Table II), is just what would be expected. For 1·35 is approximately the mean between our previous results in the first two forms and our results in the fourth form (which are respectively 1 : 1·79 and 1 : 1·15). Also the ratio obtained in the second form (Table III) may be said to be quite normal, although the film was here interrupted.

V. *Retentiveness after a considerable lapse of time.*

We further tested the pupils as to what they remembered of these experiments after the lapse of ten months. We found that the results of the two methods were now practically the same. We thought that these results should be given as a matter of form. They do not however afford a pure basis for comparison, for lantern-slides are used frequently in ordinary educational routine, whereas films are used but rarely. Thus confusion might be expected to arise more freely with the slides than with the film. For the same reason the basis of remembering is likely to be different in the two cases. The first descriptive essay was written by our subjects a week after the original presentation. In the case of the lantern-slides, the memory of this description played a very large part in later recall, and was relied upon to guard against the confusion which might naturally have arisen. But in the case of the films, where the experience had a much more isolated character, the process of recall was correspondingly more direct.

VI. *Results of an oral explanation.*

In conclusion we have tried to obtain some data as to the influence of oral explanations on the pupils' description of film and lantern-slides.

If reliable data as to the comparative merits of the two methods are to be obtained, it is, indeed, objectionable to allow them to be affected by explanations, but then, in practice, explanations are rarely withheld, so that it seems useful to apply this test also.

A year after our first investigation we submitted to the test another batch of pupils of the two lowest forms in our school using the identical film and lantern-slides, this time preceded by oral explanation. To one of the three divisions of the first form we gave only an oral description.

Comparing the numbers of main facts remembered in the five divisions, we obtained the following average results per pupil:

Film with oral explanation	6·9 facts.
Oral description only	6·3 ,,
Slides with oral explanation	6·9 ,,
Film without oral explanation ...	5·6 ,,
Slides without oral explanation ...	7·7 ,,

Naturally these data need checking by further experiments. It is probable, however, that the results of the film and slide presentations are practically the same, if accompanied by oral explanation.

Our investigation at the same time brought out the fact that the number of impressions exclusively obtained from oral description did not fall materially below those obtained when pictorial presentations accompanied oral explanation. We noticed, too, that those pupils who reported the largest number of facts after visual presentation greatly improved when oral explanations were added, whereas, curiously enough, those who reported but few facts did worse on the addition of oral explanation.

VII. *Analysis of the descriptions.*

As has already been mentioned, it is of great importance to ascertain not only the ratio between the facts actually observed and those which might have been observed, but also to test the reliability of observations. We have obtained data about this reliability by establishing the ratio between accurate and wrong observations. As before, we took only those facts which occurred in both the film and the lantern-slides. We made no deductions of the obvious facts from the accurate observations, for the simple reason that these latter had sometimes been wrongly observed. We determined the reliability of the first eight experiments in the two lowest forms separately. In the four film experiments we obtained $\frac{\text{a(ccurate)}}{\text{w(rong)}} = \frac{385}{16} = 24$; in the lantern-slide experiments the ratio was $\frac{514}{14} = 37$. In the experiment with the interrupted film in the second and third forms we obtained $\frac{267}{16} = 17$; the continuous film in the third form yielding $\frac{190}{8} = 24$; the lantern-slides in the second and third forms $\frac{251}{6} = 42$. We therefore found in experiments with the film for each wrong observation respectively 24, 17 and again 24 accurate observations, average = about 22; with the lantern-slides, on the other hand, for each wrong observation we obtained respectively 37, 42 accurate observations, average = 39. When the film is stopped repeatedly, the ratio does not improve; on the contrary it is our experience that even worse results are obtained: for one wrong observation there were only 17 which were accurate. The difference between 24 for the continuous film and 17 for the interrupted one does not strike us as considerable enough to justify any reliable conclusion. The difference between 22 and 39, the second number being nearly double the first, clearly shows that the reliability of observations obtained by lantern-slides is almost twice as great as that obtained in the case of the film.

When the nature of the mistakes made is taken into account, the superiority of the slides is still more marked. Whereas the mistakes made in describing the subjects shown on the slides mostly consisted in merely giving the facts in erroneous succession, in the case of the film the facts themselves were repeatedly described erroneously. Thus in the pictures on rice-culture the lantern-slides were responsible for only two serious mistakes due to faulty observation: in one report the rice was supposed to be transplanted to a dry instead of a flooded field, in another it was pounded on a large stone instead of a rice-block. Mistakes of a similar serious nature occurred frequently in descriptions of the film pictures; *e.g.* in one report the harrow was compared with the European harrow instead of a rake; in two others the dry field was ploughed; again, horses took the place of buffaloes; in one instance the plough was drawn by eight oxen instead of two buffaloes. According to some pupils the sowing of the rice had taken place on dry land, and two others, evidently, had arrived at the conclusion that the crops were carried home in some vehicle instead of being dragged by the help of a yoke. These and other similar mistakes made us think at times that the film had entirely failed to achieve its object, in not bringing to the juvenile mind the wide differences between rice-culture and the European manner of agriculture.

In descriptions of Batak life curious mistakes were made. What strikes us most about the Bataks is the elegance of contour and the monumental grandeur of their dwellings. This had been observed by many of the pupils, but two of them curiously enough stated that the dwellings were rather stunted and insignificant. Our belief is that such numerous mistakes in the essays are mainly due to the fact that the attention of the observer is mechanically centred on the moving objects of the film.

This is corroborated by the spontaneous utterances of some of the children. Thus one of the boys, in dealing with "the Visit to the Tea-plantation," remarked that the visitor first rode through beautiful scenery, about which, however, there was not much to tell, because it all happened so quickly. So, too, a girl, commenting on the same film, wrote, "I do not remember much about it, but this much I know; there was a gentleman constantly moving about."

It was, of course, of great interest to ascertain how the children described the movements exhibited in the film; what impression, *e.g.*, a dance made when only one picture had been shown. It stands to reason that in such cases better results were to be expected from the film than from the slides. This expectation was confirmed in many instances, but it cannot be denied that frequently the movement did not arrest the children's attention, and also that a few children described the movements out of their own imagination. Our control-experiment enabled us to establish the fact that often the same children had noticed movements in both film and slides, which goes far to prove that not only the movements on the film but the mental attitude of the child comes into play. Children endowed with lively imagination will require no film picture to assist them in fancying actions, while apathetic children will not observe movements in the busiest of market-places or at least will not deem them worth mentioning. One of the girls, for example, in her comments on the dances of the Bataks, of which only one photo had been exhibited to her, wrote: "The women perform funny dances, in which their hands are more actively engaged than their feet." On the film, however, the different position of arms was shown by six women, but this was unnoticed by all the children who had seen the film. Another girl writing about "the pounding of the rice" as shown on the lantern-slide, stated, "the young girls pound the rice, while a cackling fowl is walking about."

Without summarising systematically all the movements occurring in our films, we may conclude from a comparison of these first experiments regarding this important point that movements are not more frequently nor more correctly described from film-pictures than from lantern-slides.

VIII. *Summary.*

Summarising the results of our investigations we may make the following observations:

1. The essays written on the subjects of the lantern-slides were considerably longer than those on the subjects of the film. The difference became even more marked when only facts of didactic value were taken into account and obvious facts and those of secondary importance were disregarded.

2. Not only the average results of permanent and essential value proved the lantern-slide to be the better apparatus; they also showed its superiority when we considered our experiments with each pupil separately. Each child had evidently profited more by the lantern-slides than the film.

3. If the ratio between the faulty and the correct observations in the essays is taken as a basis of computing the reliability of results obtained, the lantern-slide proved to be superior to the film.

4. To all appearances there is no material difference between the continuous and the interrupted film, as compared with the lantern-slide, notwithstanding the fact that the interrupted film would seem to combine the advantages of film and lantern-slide.

5. It is of especial importance to notice that not only the objects and actions shown by each method were on the whole better observed in the case of the lantern-slides, but also that features which are usually considered typical of the film pictures quite as effectively arrested the attention of the pupils, occasionally even more so, in the lantern-slide.

6. To children endowed with a lively imagination the lantern-slide affords an opportunity for the play of their fancy. Children who are not gifted in this way do not appear to be more inspired by the lantern-slide than by the film.

7. Memory is not always affected in the same manner and to the same degree by the lantern-slide and by the film. The difference decreases as the children advance in years. With children of sixteen, the difference becomes so small as to be almost negligible.

8. The practical importance of our investigation lies in the conclusion that for young children lantern-slides suitable for instruction are more valuable than the film. This superiority invariably grows smaller as the mind becomes more mature. It follows that the difference observed by us is partly due to the method of projection, partly to the mental age of the pupil.

Our investigations have shown that the energetic propaganda made for the film on the strength of its alleged didactic importance is not well-founded. On the other hand, its educational importance has not been disproved.

The opinion of those who hold that the lantern-slide is to be preferred to the film is supported by our results in so far as they put beyond a doubt the great didactic value of the former for pupils up to the age of seventeen, *i.e.* for the majority of our school population.

(*Manuscript received* 16 *July*, 1923.)

PUBLICATIONS RECENTLY RECEIVED

Theories of Memory. By BEATRICE EDGELL, M.A., PH.D. Oxford: At the Clarendon Press. 1924. pp. 174. 7*s.* 6*d.* net.

This is a most admirable little book which all students of psychology would do well very carefully to study. It is, indeed, a pity that Dr Edgell did not deal more at length with the experimental side of her subject, for most of the doctrines which she has elected to consider are, in their original form, quite easily accessible, whereas much important experimental work is difficult for the ordinary reader to get at. However for its study of the biological doctrines of memory, of the views of the English philosophers from Hobbes to Spencer, of the opinions of the "new realists," of the work of Bergson, and finally of the psychology of the memory image, the volume is a valuable one. Miss Edgell's attitude is very critical, and no doubt is rightly so. But there are occasional curious lapses as when, in discussing Bain she comes upon the view that ideas have a tendency to become sensations. "This," she says, "is of course true in a literal sense for ideas of movement." Here she might well have pushed her criticism further. The matter is less clear than her words indicate, and there is no good reason for supposing that ideas of movement straightway of their own accord pass over into sensations. Miss Edgell's own explanations tend to put the main burden of memory upon conational processes which so organise cognitive processes that the latter come to be both representative and indicative of the past. The solution raises a number of perplexing problems to the further consideration of which it is greatly to be hoped that Miss Edgell will soon return.

A Comparison of Different Shift Systems in the Glass Trade. By E. FARMER, M.A. Industrial Fatigue Research Board, Report No. 24. London: H.M. Stationery Office. 1924. pp. iv + 24. 1*s.* 6*d.* net.

In this report, an investigation on the effects on output of a reduction of the hours of work in various glass factories, from a ten hour shift to an eight hour shift, shows definitely that the relative hourly efficiency in bottle blowing was increased by the shortening of hours. This increase was not sufficient to bring the output of the eight hour shift to the level of that of the ten hour shift. However, the output for a day of three eight hour shifts was considerably greater than that for a day of two ten hour shifts. Thus the shorter shift system is definitely advantageous.

The seasonal variation in output appears to be considerable in this industry; the temperature of the air and the output are inversely correlated. It is suggested that an investigation with the kata-thermometer would be desirable, with a view to providing the best system of artificial ventilation.

Intelligence Measurement: A Psychological and Statistical Study Based upon the Block Design Tests. By S. C. KOHS, PH.D. New York: The Macmillan Company. 1923. pp. xi + 312.

As a deduction from his theory that intelligence may be defined as the 'analytic-synthetic' power of the mind, Dr Kohs has prepared a performance test which he considers should be a measure of such a power. The test consists of the reproduction with colour blocks of a printed colour design. The results of testing yield a high correlation with the results obtained from the Stanford-Binet and other intelligence tests. But the probable error of the mental age determined by the block design tests is sixteen months; and the number of individuals tested of chronological ages below seven and above sixteen does not seem to have been sufficiently great to give very reliable results for these ages. It is claimed, however, that this test has proved very useful in the segregation of 'border-line' feeble-minded cases; and doubtless in conjunction with other tests of intelligence it will prove of diagnostic value.

The Psychology of Education. By D. Kennedy-Fraser, M.A., B.Sc. London: Methuen and Co., Ltd. 1923. pp. viii + 201. 6*s*. 6*d*. net.

This book provides a useful practical treatment of the problems of education for readers with no special psychological training. It is divided into four sections dealing respectively with the relation of heredity to education, perceptual learning, motor learning, and such special topics as the thought process and the psychological basis of school discipline. The criticism might be made that the author is too much swayed by the older 'faculty' psychology, while neglecting certain important recent theories such as that of the 'unconscious.' But his practical conclusions, epecially those with regard to mental testing, learning processes and discipline, seem very sound.

A Laboratory Manual in the Psychology of Learning. By William Henry Pyle, University of Missouri. Baltimore: Warwick and York, Inc. 1923. pp. 161. $1.60.

This manual, the outcome of fourteen years of experimentation, presents a comprehensive treatment of the experimental investigation of learning processes, including motor and semi-motor learning, various methods of learning nonsense material, unrelated words and poetry, and ideational learning. Full data and methods of treatment are given, together with numerous questions to be answered by the student himself from a study of the results. So methodical and detailed a treatment of the subject should be of great use to those who wish to study it closely. It is suggested, however, that considerable experience would be needed to interpret fully these extensive data.

Human Nature and its Remaking. By W. E. Hocking, Professor of Philosophy in Harvard University. New Haven: Yale University Press. 1923. pp. xxvi + 496. 18*s*. net.

It is pleasing to find that this thoughtful and suggestive work by Professor Hocking has now reached a second edition. The author has not made a very large number of changes, but those which he has introduced are, many of them, of direct psychological interest. He devotes, as would be expected, a little more attention to Freud, pointing out briefly the ethical significance of certain of Freud's views. He also deals to some extent with Dewey's book on *Human Nature and Conduct*, and he includes a paper republished from the *Journal of Abnormal Psychology and Social Psychology* on *The Conception of Instinct*. The book is mainly ethical in its emphasis, but Professor Hocking's study of the equipment of "the natural man," and many of his remarks about society are of the greatest interest to the psychologist.

Aberrations of Life. By J. C. McKerrow. London: Longmans, Green and Co. 1923. pp. 108. 6*s*. net.

This is a sequel to the same author's book entitled *The Appearance of Mind.* It is an attempt to apply the general theory of that work "to some of the abnormal phenomena of life and 'consciousness.'" It is written with lucidity and vigour and will be of interest to the psychologically minded reader who has at the same time a predominant bent in the direction of somewhat general speculation.

Papers on Psycho-Analysis. By Ernest Jones, M.D., M.R.C.P. (Lond.). Third Edition. Baillière, Tindall and Cox. 25*s.* net.

Although the present edition contains five new chapters, the work is roughly of the same length as the previous edition, owing to the omission of several papers. The remaining chapters have been revised. Of the new chapters, that on "Recent Advances in Psycho-Analysis" deals with a number of the most striking advances of the past few years. Interpretations of normal and abnormal behaviour are suggested on the basis of an infantile narcissism, or direction of the libido to the self. Dr Jones also outlines in this paper Freud's recent attempts to construct a dynamic psychology, which he calls 'metapsychology.' Perhaps the most important and original addition to the volume is a paper on "The Nature of Auto-Suggestion." Dr Jones reaches the conclusion that "Suggestion is essentially a libidinal process..." and Auto-Suggestion is founded on that primary narcissism, the importance of which is stressed elsewhere in this edition. The new material and alterations in this volume are welcome, though it is perhaps unfortunate that some of the chapters of the earlier edition have been omitted.

A Critical Examination of Psycho-Analysis. By A. Wohlgemuth, D.Sc. (Lond.). George Allen and Unwin. 10*s.* 6*d.*

It is unfortunate that Dr Wohlgemuth makes his attack on 'Psycho-Analysis' depend upon the principle that "ce n'est que le ridicule qui tue." In spite of this blemish on what purports to be a "critical examination," Dr Wohlgemuth has performed a useful service in showing how frequently the Freudian constructions violate the rules of scientific method. A number of excellent illustrations are given of such infringements, particularly in relation to the interpretation of dreams. Dr Wohlgemuth is very severe on the 'Unconscious,' mainly on the ground that it is a contradiction in terms. It seems that Dr Wohlgemuth is confusing 'Unconscious' with 'Unconsciousness.' In dealing with the contention of many psychoanalysts that the cures effected by their method are proof of the correctness of their doctrine, Dr Wohlgemuth makes out a case for the paramount influence of 'suggestion' in those instances where psychoanalytic treatment has been successful. The book is most readable. If it can hardly be regarded as a refutation of 'psychoanalysis,' it does at least reveal how naive, in many cases, is the logic employed by the 'psychoanalytic school.'

A Psychological and Poetic Approach to the Study of Christ in the Fourth Gospel. By Eva Gore-Booth. Longmans Green and Co. 1923. pp. xv + 363. 8*s.* 6*d.* net.

This book might with greater accuracy have been entitled "A Metaphysical Approach to the Study of Christ in the Fourth Gospel." The writer's concern is never psychological. A metaphysical view of personality, which strikingly resembles the theories of Theosophy, is developed in the first part of the book, and applied with the aid of symbolic meanings to the 'interpretation' of the Christ of the Fourth Gospel. It is an interesting example of the ingenuity with which religious documents, especially those of a mystical strain, can be interpreted so as to conform to the point of view and experience of the interpreter. The trouble is that there is no end to the possible 'approaches' and 'interpretations' from the metaphysical side.

Bergson is twice misspelt *Bergsen* on p. 88, and the word *heretical* is apparently confused with *heterodox* on p. 136, where *heretical* opinions are said to be "the opinions of 'others'."

As a mystical contribution to theology the book is of great interest, and contains many valuable suggestions. But it cannot be said to make any direct contribution to psychology.

My Commonplace Book. By J. T. HACKETT. London: Macmillan and Co. 1923. pp. xxiv + 438. 12*s*. 6*d*. net.

This is a fourth edition. The large and miscellaneous collection of extracts from various writers—mostly famous ones—which it contains must therefore be accepted as popular. The collection is indeed a very good one, though it is put together in a rather haphazard fashion. Mr Hackett adds a number of notes. The longest of these concern philosophic, sociological, and psychological topics. But the longest notes are by no means the best.

NOTES ON RECENT PERIODICALS

Vorträge über angewandte Psychologie

gehalten beim 7. Kongress für Experimental-Psychologie.

(Marburg, 20–23 April, 1921.)

Rechtlinien für die Methodik der Psychologischen Praxis. (W. Stern.)

This lecture deals with difficulties of and developments necessary in experimental psychology as applied to vocational tests—the supplementing of tests by the results of long observation, the advantages and disadvantages of mass and individual tests, the combination of 'mosaic' with 'structure' methods, the diagnostic and prognostic value of the tests, the effect on the latter of the factors of practice and development, of surrogate or compensatory functions, the evaluation of test results, their later confirmation or otherwise in practical experience, and their immediate confirmation or otherwise as a result of application to those already satisfactorily occupied.

Allgemeine und kritische Bemerkungen zur Begabungs- und Eignungsforschung. (Otto Lipman.)

A lecture along lines similar to the preceding, but more critical of the scientific value of vocation and qualification research, and laying particular emphasis on the dangers of 'practical' as contrasted with 'applied' psychology.

Grundsätzliches über Eignungsprüfungen. (Hans Rupp.)

This writer is concerned in greater detail and less theoretically with many points mentioned in the above articles and some additional ones, *e.g.* the choice of tests for any particular vocation—these should deal with the 'critical' points of such a vocation, which must be determined by a 'subjective' examination of the vocation, a complete analysis not being required, and while not necessarily reproducing actual conditions of the occupation, should be as near them as possible. Particular difficulties of a task, which must determine tests, may belong to separate activities or easily separable details of activities or depend on more general conditions. The author points out the pitfalls of relying on correlations, and the necessity of attention to variations and their cause, the effect of practice, the avoidance of tests whose chief difficulty disappears after a single solution, etc., and lays special emphasis on the necessity and difficulties of confirmation tests.

Eignungsprüfungen für Telephonistinnen. (Hans Rupp.)

We now come to the practical application to selection tests for telephonists of considerations brought forward in the above. Starting from a test of speed in the correct placing of numbers on a squared board, experiments are being carried out to discover what variations of this method provide the most reliable results, *i.e.* are least subject to variation, effects of practice, and unsatisfactory correlations (alterations in rank in tests carried out at different times). Further, a series of experiments is also in progress to determine the best method of measuring the maximal attainments of employes in their actual occupation.

Zur Psychotechnik der Arbeitshand. (Fritz Giese.)

This article is more concerned with the possibilities than the present attainments of a psychological study of the hand regarded in a wide sense as the working instrument. The author insists on the necessity of regarding the hand rather in relation to the working Ego than to the object (machine, etc.), in respect in the first place of both position and type of movement, secondly of precision or quality and tempo or quantity of work—the latter being divisible into spontaneous and imposed tempos. This division allows of research into speeds most suited to particular individuals, and a similar division must be made in the enquiry into work-curves, which shew the different effects of 'drill' and of mere repetition. The author then considers the subjective standpoint of handwork as an experience of the individual, and deals with

(1) Psycho-physical adequacy, which is connected with the feeling of the 'natural' or 'obvious' and depends on (*a*) the position of the hand in repose, (*b*) the difficulty of the movement, (*c*) 'sub-conscious' associations—the tallying with instinctive movements and natural economy of movement in ease.

(2) Autonomy of the hand—different combinations of the two hands.

(3) Series acts—the effect of division into connected series as regards monotony, rhythmical connection, etc.

(4) Dominance functions—the question as to which sense or function plays the dominating role in complex acts.

Zwei neue Apparate zur Eignungsprüfung der Strassenbahner. (Gustav Kafka.)

In apparatus 1 marks representing passer-by and tram revolve round the edge of two crossed parallels, thus giving four possible points of contact in place of the single one of the apparatus previously in use.

The second apparatus is more elaborate and adapted to the more difficult task of varying the speed of the passer-by as well as the point at which the tram-lines are met. This is accomplished by means of a series of electric lights shining through slits over which runs a continuous transparent roll with a line representing the tramways. Worked on the lines of 'illuminated signs' the impression of a moving point of light is created, of which the speed can be regulated.

Zur Psychotechnik des Bremsens bei der elektrischen Strassenbahn. (Gustav Kafka.)

A criticism of the arrangement of the brakes on the Munich trams from two points of view: (1) lack of synergism of two hands at most critical moments, (2) movements required are in contrary directions from those of instinctive movements in times of danger.

Neukonstruktionen von Apparaten zur praktischen Psychologie. (R. W. Schulte.)

Aus der Psychotechnik des subjectiven Schallmessverfahrens. (Hans Rupp.)

This account of an experimental enquiry into the subjective experience of those on 'listening posts' during the war deals (1) with reaction times and their variations under different conditions, correct self-measurement of reaction times, connection between bad reaction times and mistakes in localisation, and the effect of repetition of the stimuli. Individual differences in normal cases were found to be small and to tend to disappear with practice. (2) Localisation, found to be affected by the position of the head, *i.e.* whether the ears are in a vertical or horizontal relation to each other. A tendency to localise to the left of the actual sound was connected with the reading of a scale from right to left, and converted into a tendency to localise to the right when no scale was marked on the wall behind which the sound was made; similarly where the subject was of a nationality whose method of reading is from right to left. Experiments on this matter are still proceeding.

Contrary to actual experience on the field, practice was found to have little effect on accuracy of localisation.

Zur Kritik der psychologischen Korrelationsmethoden. (Albert Huth.)

Of all correlation formulae now in use the author states that Deuchler's $\left(r = \frac{i - k}{i - k}\right)$ is the most valuable as applicable to seven different types of correlation and containing no probable errors. But he considers that it should be supplemented by use of correlation tables based on the trigonometrical values of the 'angles' of single part-correlations, thus giving exactitude for all the varieties of correlation.

Zur Analyse des Lernens mit sinnvoller Verknüpfung. (G. Hegge.)

An account and analysis is given of the method by which it was possible for a particular subject to commit to memory a series of 350 words chosen at random, and reproduce them with only 11·7 per cent. omissions.

Untersuchungen zur Methode des fortlaufenden Addierens. (A. Pauli.)

Mass experiments in continuous addition and in the separate processes concerned were carried out with the aims (1) of ascertaining both normal values and variations in respect of typical individual differences of age, race and education, (2) of analysing the process. The most important results were gained through noting the influence of repetition on the whole process and on its constituent parts (*i.e.* reading, mental addition, writing).

Zeitschrift für angewandte Psychologie.

Band 21, Heft 1–3.

Die Facultätsdifferenzen als psychologische Gruppenunterschiede bei den Universitätsstudenten. (A. Wisse.)

From answers given by nearly 400 Dutch students to a questionnaire of over 100 questions, the writer endeavours to arrive at the differentiating characteristics of students of the various faculties.

The most important result of her research she considers to lie in the possibility of dividing students into two groups: I, those for whom the study itself is of the chief importance; II, those for whom the real importance of their study is as an introduction to their future work. In the first group she puts mathematicians, scientists, philologists, some theologians; in the second, medicals, technicians, jurists, some theologians.

Bemerkungen zur Tierpsychologie. (G. Heymans.)

In any comparison between animal and human psychology, the author points out the importance of a similarity of conditions and circumstances in which case an apparently big difference may be minimised or disappear. Thus he compares the 'purposeless' actions of cats endeavouring to scratch their way out of a box when more direct means are at their disposal, with those of men when attention is very strongly focussed on the end to be attained or very weakly on the means thereto, *e.g.* in escaping from a sudden outbreak of fire. Concentration of interest and strength of the emotional accompaniment decide as to the 'purposefulness' of an act, rather than amount of intelligence. The less convulsively fixed is the psyche on the end to be attained, the greater the possibility of variation of means to that end by emergence of a greater number of ideas. Rather than an intellectual defect in animals, the writer sees a limitation of interests and a fixing of attention on the necessities of life, and that which is directly connected therewith—thus, that they are almost in the condition of a deeply pre-occupied man.

Die literarischen Neigungen in Kindes- und Jugendalter. (Walter Quast.)

From answers of about 3600 pupils, aged 8–18, to questions as to books most favoured or disfavoured and why, the writer distinguishes stages of literary taste fairly definitely characterised. At the conclusion the results are given graphically in curves of preferences for fairy-tale, myth, tales of adventure and heroic deeds, 'girls' books,' novels and technical reading for the two sexes, and for the different types of school investigated.

Band 21, Heft 4–6.

Beiträge zur Psychologie der Übung. 2–4. (Annelies Argelander.)

2. Effect of Practice on Quality of Work Done.

Experiments to test the above were carried out in connection with correct measurement of space by touch, movement and stretch of the hand, and shewed a difference in result in measurement by movement and stretch according to the type of subject (motor, static). In quantitative performances physiological factors are of greater importance than in qualitative work, where individual differences have greater effect, especially interest and antagonism.

3. Here the effect of practice on quantity of work done was tested by the substitution of other letters, according to a key provided, for those in a given series of words. Results on an average confirmed the supposition that a high commencing speed improves slightly, a medium speed moderately, a low speed considerably, with practice.

4. The connection between initial performance and improvement with practice was further investigated in actual industrial work from material supplied in connection with groups of apprentices. Here, as in the above, considerable alteration of relative positions in the group was found, and the same relation between initial performance and improvement. Results as a whole went to shew the unreliability for future production of a choice of apprentices after a single test.

Zur qualitativen Bewerkung des Zeichentests von Binet-Simon. (Jakob Bappert.)

In using the Binet-Simon drawing test, it is found that children have much greater difficulty in drawing a rhombus than a square. Additional tests to discover the cause of this shewed that drawing of the rhombus nearly always began with the upper half, and that it was the second half that gave the greater difficulty, not only as the second drawn, but as the lower. Further investigations pointed to the connection of the difficulty with the development of motor 'imagery,' drawing movements proceeding with greater ease in certain directions, these lines of direction not being regarded as isolated, but in their relation to the figure as a whole. The earlier development of motor than visual imagery and the influence of the former in the formation of the latter is emphasised. Backwardness in reading is found in high correlation with difficulties in drawing the lower half of the rhombus, a fact which points to the connection of the difficulty in grasping the word as a whole with poorness of spatial visual imagery, and the importance of motor imagery in structure formation.

Zur Theorie der Sekundärempfindungen und zur Bleulerschen Theorie im besonderen. (Karl Lenzberg.)

This criticism of the Bleuler theory of secondary sensations on both theoretical and empirical grounds concerns itself with the phenomenon of colour-hearing. The writer finds that Bleuler has in no way weakened the case for the ascription of specific energy to the sense nerves with corresponding sensations as a result of appropriate cortical stimulation, in favour of the assumption that such phenomena as colour-hearing result from a general cortical stimulation always present, but in this case with less complete predominance than usual of one over all other types of sensation produced.

From his experiments with a chromo-phonoptic boy of 11½ years he finds

(1) no regular correspondence of one tone and one colour,
(2) no absolute sense of pitch,
(3) equal limits of error for pitch and correspondence of tone and colour,
(4) a certain independence between photism and sound as regards appearance and disappearance.

He concludes from these and other experiments that photisms can be classed as 'ideas,' that the connection between deep tones and black, high tones and white can be explained by Wundt's hypothesis of similarity of feeling tone, but that the choice of colours for intermediate tones is bound up with their recognition and the result of deliberation on the degree of brightness corresponding thereto.

Über audition colorée. (Geza Revesz.)

This writer, in dealing with the connection between colour and number, had the advantage of a 7 year old chromo-phonoptic daughter with an unusual gift of self-expression and self-observation. As a result of research with her and with many other subjects, he makes his chief contribution to the question in the direction of classification. He suggests five classes, according to

A. The extent of the number-chromatism (10 is the limit in the majority of cases).
B. The continuity or discontinuity in incomplete cases, discontinuity probably indicating the influence of chance association. (1) Gaps may be filled by virtue of similarity, (2) or by deliberate memorising, (3) or by rational procedure, in these cases giving an apparent continuity. (4) Occasional cases of original continuity.
C. Difference of primary impressions or ideas—connection of photisms with (1) phonetic number word, (2) optical number picture, (3) number form, (4) number idea, (5) number series.
D. Method of appearance of number and colour, *e.g.* number itself coloured, appearance on coloured ground, with colour spot at side, imagined and not visualised, etc.
E. Originality or derivation of chromatisms.
F. Consciousness or latency of chromatisms.

The writer further points to indications of the formation of chromatisms in the years when 10 is the limit of familiar acquaintance with numbers.

Band 22, Heft 1–2.

Die Entwicklung der Zahlleistung bei 2 bis 6 jahrigen Kindern. (Hermann Beckmann.)

The only article in this number deals with an enquiry into the capabilities of children in half-yearly stages between 2 and 6 with regard to counting, and particularly with the biogenetic developmental processes, *i.e.* those undisturbed by set teaching.

Band 22, Heft 3–4.

Über das Zustandekommen der Irrtümer in der Wahrnehmung. (A. Schweisgut.)

The investigator exposed in a tachitoscope about 15 pictures for 1/5 sec., 2–3 at a sitting, to 20 subjects who immediately gave as full an account as possible of their contents; errors were pointed out, and an endeavour made by means of questions and self-analysis of the mechanisms to arrive at their cause and origin. The following classification of errors was used:

A. 1. (*a*) Interpolations, (*b*) omissions.
  2. (*a*) Qualitative, (*b*) quantitative alterations.
  3. Alteration of spatial arrangement (*a*) incorrect position of otherwise correct observation, (*b*) otherwise correct observation incorrectly extended, (*c*) change of sides.

B. Chiefly objectively conditioned and due to (*a*) point of attention—observation more correct in breadth than height, and attracted by strong lights, etc.; (*b*) arrangement of light and shade; (*c*) optical illusion—*e.g.* brighter portions seem more to the fore and larger than darker; (*d*) change of sides—the conditions for this being two distinctly separate sides and a certain symmetry of grouping; (*e*) content of picture.

C. Subjectively conditioned by
  (*a*) Personal attitude of individual—effect of recently acquired impression and of emotional tone.
  (*b*) General attitude—(1) Associative falsification along lines of familiarity. (2) Falsification through the tendency to attribute meaning. An interpretation arising from the perception of a single detail is without objective justification extended to the whole picture. (3) Interpretation proceeds step by step, depending on all the elements of the picture, with or without a hesitancy in judgment.
  (*c*) Tendency to exaggeration.

Both quantitatively and qualitatively it was possible to connect errors with certain types of picture—object—and with certain types of subject. The latter can be grouped as follows: (1) those who describe copiously, whose description proceeds from a grasp of the whole to details and with a withholding of judgment; (2) those giving scanty description and making few errors; (3) those making more errors but describing more freely.

The author stresses the importance of such research in application of the results to objects of demonstration in scientific and school work, and in the domain of aesthetics.

Geistige Leistungen und psychisches Milieu mit besonderer Berücksichtigung der sozialen Schichten. (Luise Habric.)

The tests first used by Benary for selection of airmen and later by Kafka for engine-drivers are here described as adapted for intelligence tests. They consist in picking certain indicated forms out of patterned cards in three grades of difficulty. The author found that quickness and accuracy were on the average inseparable, that girls, especially those aged 13, were superior to boys of the same age (though in experiments with adults the results were reversed), and particularly that lesser intelligence was related to lower social grade—again, of course, in the average. Results of the experiments themselves were supplemented by questions as to occupation, and went to prove the effect of milieu as stimulus on intellectual attainments. The greater number of experiments were carried out on children of 13 and 14 belonging to different types of schools.

Mental Growth Curve of Normal and Superior Children: Studies by Means of Consecutive Intelligence Examinations. By BIRD T. BALDWIN and LORLE I. STECHER. University of Iowa Studies. First Series, No. 56, January, 1922.

This investigation deals with the details of the curve of mental growth of superior and average children of both sexes, the possibility of prediction of mental growth, and its relation to physical development. The data were gathered by consecutive testings at intervals of a year, of 143 children, 56 of whom had two examinations, 51 three, 42 four and 36 five. On the whole the constancy of the I.Q. is confirmed, although there appear to be certain fluctuations associated with physical growth. Considering each child's deviation from his mean I.Q. expressed as a percentage of his mean I.Q., there is a tendency for girls to be more variable than boys, for the superior children to be more variable than the average children, and for the older children to be more variable than the younger. Considering the difference between the first and second examination the larger number of cases show a difference of less than five points (+ or −), in I.Q.; between the first and other later examinations with intervening practice, many more cases show greater amounts of difference, the positive differences becoming more and more marked. With the change expressed as a percentage of the previous I.Q., there is a slightly greater change in the positive direction for superior children, due probably to the fact that these profit more readily by practice. Superior and average children develop at different levels, and grow increasingly dissimilar in mental age with increase in chronological age. The general prepubertal increase appears earlier in the case of superior children, and an adolescent superiority of girls is in accordance with other facts indicative of their earlier maturity. The coefficients of correlation between all examinations within the four groups are high and reliable, ranging from + ·72 ± ·05 to + ·93 ± ·02, showing that they may be used as a basis for prediction.

The authors hold that they have empirically established a close interrelation between mental and physical development as shown by the general similarity between growth in height and in mental age, the rise in the mental age curve at the adolescent years, and the superior mental development of physiologically accelerated children. They also conclude that the use of the Stanford Revision of the Binet scale even for the relatively short period of four years shows the unsuitability of this scale in its present form as a means of measuring mental growth. The limited number of alternative tests results in a certain practice effect on repeated examinations. Another defect is the lack of a sufficient number of tests at the higher levels to measure the mental growth that apparently goes on in a bright young child even after the exhaustion of the 16 or 18 year old tests. Theoretically it would seem to be a better measure of mental growth to use a combination of point scales for specific mental traits, each scale to be sufficiently extended to measure whatever ability exists and the whole system to include a sufficient variety of traits to afford a general measure of the development of the individual.

Psychological Monographs. No. 1331.

A Socio-Psychological Study of Fifty-three Supernormal Children. By WILLIAM T. ROOT, Jr.

The fifty-three children studied were selected on the basis of two factors, viz. (1) Binet test scores, in which the minimal requirement was an I.Q. of 135 in the ages 7 to 12, and of 120 in the ages 13 to 15; and (2) special ability along some line. The psychological examination, in addition to the Stanford Binet test, consisted of a series of selected tests, in which the superior group was compared with an established norm.

The superior group, throughout, surpassed the norm group both in the Binet and in selected tests; but in both, the contrast decreased with increase in age.

There is a suggestive analysis of the home conditions of the members of the superior group, in relation to the facts brought out by the tests.

University of Oregon Publication. Vol. I, No. 11, July 1922.

Mental Differences in Certain Immigrant Groups. By KIMBALL YOUNG.

A study designed to investigate the general intelligence of children among certain immigrant racial stocks in California, in order to discover whether the alleged language handicap in school progress is in fact operative, and to secure facts as to the general mental differences of the racial stocks studied. The psychological tests used were of two types, verbal and non-verbal, the army Alpha and Beta respectively; and these were compared with teachers' estimates of general intelligence and school work, with 'grade location' and with parents' occupational levels.

The results showed an extensive retardation of the Latins as compared with native-born Americans. In the teachers' estimates both of school work and of general intelligence, the non-Latins exceed the Latins, in terms of mean rating, by an amount equal to nearly one-seventh of the range of the scale upon which the ratings are made. The Americans are nearly two grades ahead of the Latins, as regards 'grade location.' The economic status of the parents of the children shows that there is a rough positive relation between economic class and mean intelligence of the children. With the Alpha test, the upper 50 per cent. of the Latins scarcely exceed the lower quartile of the Americans, while only from 5 to 7 per cent. of the Latins exceed the median of the Americans, and but 1 to 2 per cent. exceed the upper quartile of the latter. With the Beta, only from 11 to 16 per cent. of the Latins exceed the median of the Americans, from 25 to 30 per cent. exceed the lower quartile of the latter, and but 4 to 7 per cent. the upper quartile. This is considered to be proof of the decided inferiority of the Latin stocks. The correlational materials are held to show that the verbal test, Alpha, is a better measure of the ability of the Latin groups than the Beta, and that therefore the alleged language handicap does not exist to anything like the extent supposed.

The study concludes with a general discussion of certain theoretical features of race differences and cultural progress, a discussion which is unfortunately not entirely free from bias.

VOLUME XV JANUARY, 1925 PART 3

THE NEW PSYCHOLOGY OF 'SHAPE'

BY C. SPEARMAN.

I. *The general movement* (pp. 211–212).
II. *'Acts' versus 'configurations'* (p. 213).
III. *Quasi-physiology* (pp. 213–214).
IV. *Needless introduction of 'faculties'* (pp. 214–215).
V. *Needless introduction of metaphysics* (p. 215).
VI. *Legitimacy of analysis into relations* (pp. 216–219).
VII. *The question of analysis into separate processes* (pp. 219–221).
VIII. *Original perception* (pp. 221–223).
IX. *Conclusion* (p. 223).
Bibliography (pp. 223–225).

I. *The General Movement.*

AT the present moment, especially in German-writing countries, scarcely any topic is causing more stir among professional psychologists than the attempt to reconstruct the science of what has been variously named 'shapes' (Gestalten), 'forms,' 'wholes,' 'complexes[1], 'complexions' or 'structures.' In the hope of unequivocally denoting the abstract shape itself, considered apart from any object so shaped, such further names have been introduced as 'shape-quality,' 'shape-moment,' or 'complex-quality.'

Typical of what these terms signify—the word shape indicates this—are spatial figures, as circles, triangles, etc. But the more or less synonymously used words, whole, complex, etc., show that the meaning has by analogy been extended vastly further. Indeed, no example has been more often quoted as representative of 'shape' than musical melody. In order to mark out how much the word is intended to mean, it has been formally defined as comprehending "those structures which as wholes possess specific properties and therefore can with good right be regarded as unities[2]." This laudable effort to avoid ambiguousness has not perhaps been very successful in practice. But evidently enough, the cognising of shapes is taken to have an extremely wide scope; it appears to cover all or nearly all of the most vital operations in sensory perception. Its sphere has even been extended throughout the range of thought also.

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[1] These latter, of course, have nothing to do with the complexes of psycho-analysis.

[2] *Physical Shapes at Rest*, by Köhler, 1920, p. ix.

We are confronted, in fact, with little less than an atten
the psychology of cognition.

If this imposing movement is to be traced back to any
the best claim is undoubtedly on behalf of Wundt with l
principle of 'creative synthesis.' But a subsequent writin
fels, seems to have been more directly stimulative. In
other very important publications were made by Mei
Stumpf, Kreibig, Marty, v. Kries and Cornelius. The
shapes played a fundamental part in the great work of
Phenomena of Consciousness. Some of the suggestions ma
had the good fortune to obtain the powerful support a
developments of Felix Krueger. As for the recent contri
topic, these have been innumerable, ranging from the
of Selz to such brief but pregnant indications as those c
Lipmann. A particularly brilliant and proselytising g
formed under the leadership of Wertheimer, Köhler, and
spicuous on the solid side of experiment have been the
of Schumann and Linke, the beautiful investigations c
monumental research of Bühler, the delicate analyses c
and Katona, the insightfull investigations of Krueger's sc
as represented by H. Volkelt, F. Sander, and G. Ipsen,
the peculiarly significant pathological inquiries of Popp
stein, Gelb, and Fuchs. Most dramatic of all has been
the lists last year by G. E. Müller, who proved that r
without taking from the world-famous acuteness of his vi
to it a new degree of breadth.

In general, the trend of this great movement has bee
the previously more or less prevalent doctrine of assoc
latter had sought to trace back all operations of knowing
of sensations and of associative reproductions. Such na
now declared to be utterly inadequate. In truth, though t
to have been little noticed, these modern authorities a
back to that ancient distinction—already a plank in t
Plato—that was made between the ὕλη on the one han
or μορφή on the other.

In one of its features, however, this attempted refor
considered unsatisfactory. Instead of pressing home
attack, most of these shape-psychologists have been n
turn aggressively against one another. The labour th
been devoted to establishing the largest possible qua
scientific knowledge has been dissipated in controversi
sundry points of disagreement.

II. *'Acts' versus 'configurations.'*

Now a large proportion of these differences of opinion, the present writer believes, could well be treated in a more opportunist manner. Instead of being allowed to monopolise scientific effort at the expense of positive progress, they could quietly be noted and put aside for eventual settlement upon convenient occasion.

But at least one divergence of doctrine does appear to divide psychologists into so gravely conflicting camps as to render reconciliation a pre-requisite for joint advance. The two contending parties must first close towards one another before either can hope to move successfully forwards.

Expressed broadly, the one view—especially advocated by Meinong and his school at Graz—still follows the associationists to the extent of bisecting the perception of shape into two steps; the first consists of sensation whilst the second is some process superposed upon these. It only departs from associationism in that it takes this second step to be no mere reproductive event, but rather a creative 'act.' The opposing school—represented in a more moderate form by Krueger and in its extreme form by Wertheimer—rejects two steps altogether; it declares the entire shape to consist in an indivisible 'whole' or 'configuration,' which all comes to mind with equal immediacy. "The sensations do not exist," it is now said, "there occurs nothing but one single perception." In a word, the quarrel is as to whether the operation of perceiving shape is in essence binary or unitary; the partisans may be called respectively binarians and unitarians.

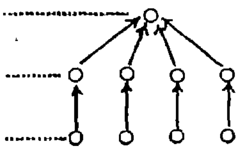
III. *Quasi-physiology.*

In considering the main points here at issue, we may conveniently begin with those where one or both of the belligerents seem clearly to have been led too far. And we may take first a propensity of the extreme unitarian school to place themselves upon physiological ground. The sensations and superposed acts which they contest would naturally involve a lower and a higher level in the cortex of the brain; the whole cortical proceeding, then, as their opponents might be expected to imagine it, could be symbolised as follows:

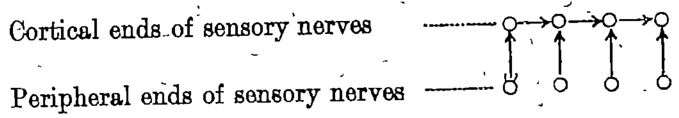
Brain level of cognition of shape

Brain level of sensation (cortical ends of sensory nerves)

Peripheral end of sensory nerves



Such a view Wertheimer proceeds to attack; he asserts that one brain level is sufficient. Account must be taken, he says, not only of the cortical ends of the sensory nerves, but also of the 'cross-function' (Querfunktion) between these. When different sensory nerves are excited simultaneously, then according to him the interconnecting cortical neurones at once short-circuit the excitation, as represented below:


Cortical ends of sensory nerves
Peripheral ends of sensory nerves

What really underlies the perception of shape, he believes, is the excitation of the sensory nerves *plus the short-circuiting*.

This doctrine has been developed by Köhler into one of extraordinary grandiose scope. Indeed, he seems to advocate a general reconstruction even of the science of matter itself.

Interesting as his exposition is, however, it appears to suffer from one serious weakness. The evidence and arguments brought forward by him are not, in the main, genuinely physiological at all. They consist for the most part in that very common and often useful, but sometimes most dangerous, translation of psychologically observed facts into physiological language. Such translations are rarely more than bold guesses. Waiving, then, all objections in detail—they have been forcibly set forth by G. E. Müller—we must for the present shelve this physiological theory as an extremely ingenious but no less venturesome surmise.

IV. *Needless introduction of 'faculties.'*

The next extremist view to be noticed here is a tenet of the other or Graz school. It consists in explaining the alleged acts as achievements of the 'intellect' or 'intelligence.'

Roused to lively opposition, the unitarians have retorted that the cognising of shape is on the contrary quite unintellectual; they have contrasted it with sensory comparison, which does, they say, possess intellectual dignity. In support, they have even ventured into the speculative waters of animal psychology. None of the lower animals, it is argued, possess intellect, and yet some perceive shape; *ergo*, to perceive shape does not involve intellect.

But whilst the two schools thus disagree as to where the 'intellect' operates they alike omit to mention what this word is really intended to mean. In truth, it is but a survival of the old faculty-psychology; as now commonly used, it has shimmering behind it meanings as numerous and diversified as have been the different systems of faculties advocated throughout history. To employ the word without ever having defined it is not to reason scientifically but rather to invoke a *deus ex machina*; it is to turn psychology into a mythology.

Something of the same reproach may be urged even against those who claim to explain the perceptual acts as due to 'attention.' For this, also, is one of the ancient faculties; it is at present being commonly handled, not as any definite idea, but as a word to be invoked when ideas fail.

But why bring in the faculties at all in order to discuss the acts? Why even introduce this last word, seeing that it is unnecessarily contentious? The really vital part of the binarian doctrine can be expressed without any such disturbing complications. It is simply that the perceiving of sensory shapes essentially involves two processes; first, the bare awareness of sensations; and then, the cognising of the shape into which they fall.

V. *Needless introduction of Metaphysics.*

There is another complication which this binarian school would appear to have needlessly dragged on to the carpet. Setting out from the doctrine that shapes or relations are not given immediately in the sensations, the corollary has been drawn that such shapes and relations cannot 'exist' or 'subsist' 'in reality.' So strongly has this deduction been advocated, that it is now being regarded as the keystone to the whole Graz theory. But their opponents are no less lavish in their appeals to 'real parts,' 'self-insufficient moments,' 'thing-like' objects, and so forth.

Surely, both parties are unmindful that this matter furnished the chief theme of discussion to many of the ablest thinkers of ancient times. In those days, a favourite example was the case of two white walls. Over and above the existence of these themselves, it was asked, does there furthermore really exist any relation of resemblance? Into this seemingly almost trivial problem, these sages burrowed deeper and deeper for many centuries, but only to find always new depths to it. And the question of 'self-sufficiency' is at least equally profound. How, then, shall these abysmal regions ever be explored by such hasty surface splashings as are offered in the modern literature at issue?

And why, after all, should any explanation ever be incumbent upon psychology? The very words 'real existence' show that the problem does not lie within the domain of mere empirical science, but belongs rather to the secrets of ontological philosophy.

VI. *Legitimacy of Analysis into Relations.*

One more point remains on which the contending parti have impulsively overshot the mark. The configurationists c not content with asserting that the perception of shapes is being analysed into separate processes, have gone so far as t it insusceptible of analysis of any kind.

Often, indeed, their language is so unguarded as to co pression that they are tilting against the very law of contra instance, they seemingly assert with a single breath, on t that analysis by experimental psychology shows a musi contain overtones, and yet on the other hand that observ these overtones *not* to be present. Evidently, either the analysis, or else the observation, has been fallacious. So analysis is true, then—on pain of immediate self-contradi constituent discovered by it in any whole does really occur i moreover, it possesses in this whole every single intrinsic c it has in the state of analysis.

Not so easy to dispose of, however, is another kindr particularised contention of the unitarians; this is a d perceptual shape can be analysed into constituent *rel* relations are indeed admitted to subsist objectively, but be no more components of the shape than, say, electro-ma are components of colour percepts, or than a penny inser slot is a component of the ticket that shoots out in conseq insertion. Thus, Köhler declares the only truth to be th usually preserves its characteristic nature when the framew of objective relations remains the same; but this, he says, i of invariance, not an explanation[1]. And here for once hi in agreement with that of many even of the Graz school. long before written that "the shape as object is indeed impos the relations, but the presentation of the shape is not b thought of the relation[2]." Indeed, even Ehrenfels had taken position. When relations are cognised, he urges, this is effe of "a thought which is not at all of a perceptual character" (

Now, in answer to this, it must necessarily be conceded t can occur in two strikingly different ways, of which the one m thought-like and the other perceptual. Thus, in the case o points arranged vertically, I may either have a thought-li of the verticality, this character then appearing distinct from the points in themselves, or else I may have a perceptual awareness, wherein the verticality and the points are intimately fused together. In the former case, the presentation of verticality is 'free'; in the latter case, it is 'tied.' But there seems to be no reason whatever why the verticality should not be regarded as a 'relation' equally in both cases. Even ordinary language has provided appropriate expressions for both. In the thought-like case, a man is said to judge *that* the points are related; in the perceptual case, he sees them *as* related[1].

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[1] This *Journal*, 1924, XIV.

[2] *Ztsch. f. Psychol.* 1914, LXIX, 281.

Moreover, the two ways of knowing appear to be at bottom only different stages of the same thing. Relations that at first have been cognised in the fused perceptual manner tend with practice of suitable kind (the focalising or abstractive mental attitude) to become known in a more distinct and thought-like way. The reverse kind of practice has the reverse effect; relations which had previously been thought of distinctly may relapse into fusion with the related material. Even the ordinary hearing of a melody exhibits all grades of transition between the two ways. The simpler and more familiar relations are usually perceived in the 'as' manner. Many of the more complex and strange relations, on the other hand, fall decidedly into the category of 'that.' But intermediate between the two come perhaps still more numerous cases. Accordingly, the attempt to erect an impassable barrier between the 'as' and the 'that' manner of knowing relations would seem to break down as unnatural and unscientific.

Conceding, then, the distinction that has been drawn between the two manners of knowing, but protesting that both equally deal with relations, we are led to realise that relations do furnish the very woof and warp of shapes or whole-qualities. For instance, any opening phrase of a tune is essentially characterised by being fast or slow, ascending or descending, moving by short steps or by long leaps, returning within the previous leap or going yet beyond it, etc. Every one of these characters, fast, slow, ascending, etc., is essentially relational. Passing to the second half of the musical sentence, this must have some sort of balance to its predecessor, whether by repetition, development, repartee, or other device known to the technical musician. Each of these again essentially involves relations. As much may be said of the melodic completion. Moreover, between all such more or less elementary

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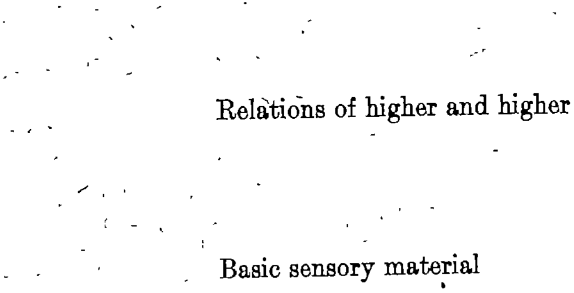
[1] All this, be it noted, is no "logical construction"—in any possible meaning of this vague phrase—but simply the most accurate description of what may be actually observed in experience.

relations, there are perceived further r
more, others of yet higher order; an
the complexity of the structure. The
perhaps that we have, Sir Walford D
pupils, "Grip the relations!"

The perceptual operation in gener



The entire figure represents a 'wh
portion above the sensory material i
can be called a whole, etc,; but more
'shape-moment,' or 'complex-quality

Why, then, it may not unnatur
shape into relations ever meet with s
to have been owing to inacquainta
The binarians had declared a shape
and this it most certainly is not.
to throw out the child with the
summation is only one out of seve
wholes may be constituted. Thoug
can quite well be called a 'system,'
to use the term very appropriately

A second argument of the anti-r
occasion be perceived when one or
is demonstrably not so. But in ma
relation really entails a proportiona
I fail to catch some relation of balan
corresponding extent I find myself
whole. As for the remaining cases,
simply made good by invention[3].

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[1] See *The Nature of Intelligence,* by the
[2] For instance, Marty, *Unters. z. Grundl*
[3] By means of 'correlate-finding,' see *T*

by exhibiting a circle with a gap so arranged as to fall within the blind spot of the observer's retina. In spite of the gap the circle will still appear to be entire.

There remains one more shot in the locker of the anti-relationist. It consists in trying to make capital out of the fact—so interestingly investigated by Krueger and his school—that the concentrating of 'attention' upon any portion of the percept tends to make the whole shape disappear. In truth, this happens merely because mental energy is withdrawn from those relations which are most characteristic of the shape. (Most often, these dominant relations are among those of higher order, see figure opposite.)

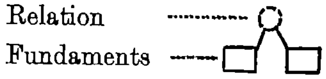
To complete this section, it may be added that shapes not only admit of analysis into relations, but do not admit of being analysed in any other way. The very authors who formally protest against such analysis are in their own actual practice continually availing themselves of it. True enough, it is only to be done by means of abstraction; but all science whatever is in just the same plight. The psychological relations are no worse off in this respect than are the 'force,' 'heat,' etc. that form the pillars of physics.

VII. *The question of Analysis into separate Processes.*

So far, we have been examining points in which either or both of the contending schools seem to have been led into exaggeration. The Graz binarians, not content with maintaining that the perception of shape involves a process over and above sensation, have gratuitously invoked mythological 'faculties,' and moreover have wantonly strayed into quagmires of philosophy. Many of the unitarians, for their part, not satisfied with denying that the perception of shapes can be analysed into two concrete processes, have gone to the length of denying that the percept can be analysed at all, even abstractly, into constituent relations.

Eliminating these extremist views, we are left confronted by the really basal question. Does the perceiving of sensory objects as related or shaped involve essentially two processes, or only one? Otherwise expressed, is the knowing of relatedness posterior to that of the items related, or are the two equally and jointly immediate? Posteriority has been advocated by the Graz theorists; joint immediacy, by Wertheimer, Köhler, and Koffka.

The issue is far more weighty than might appear at first sight. On the unitarian alternative, the analysis of shape into a complex of relations will have little more than classificatory value. But on the relation tends to be followed by a stage at which they do exhibit it. In the following figure, the continuous lines represent the initial stage, whilst the continuous *plus* the dotted lines are the final stage.



Even our typical illustration, melody, manifests this law of succession in the clearest manner. The oftener one hears, say, an elaborate fugue, the more numerous become the relations entering into the percept. Furthermore, many cases occur where the awareness of relation, instead of arising upon any repeated hearing, do so upon the first occasion but at an appreciably later moment than the tones themselves.

But when once this law of relation-finding has been established in present experience, it can legitimately—like that of the Niagara erosion—be applied to earlier periods also. And as the law implies that the sensory items have been continually enriched with relations or shapes, the deduction becomes possible that at primitive stages of development these items were comparatively unrelated and shapeless. Hence, an adult perception of shape, although seeming indeed to be only a single process, has really been derived from at least two.

But after this fashion, having set out from a great truth rightly defended by the unitarians, we are led on to recognise as also true the basal binarian proposition. The experiencing of sensations—*pace* the interpretation of this word 'sensation'—does appear to be anterior, and the cognising of relations to be mainly posterior.

VIII. *Original perception.*

Nevertheless, we are not hereby committed to the doctrine that the earliest stage of cognition consisted in a multitude of sensations distinct from one another (and corresponding to as many sensory nerves). For account must be taken of a further law, still more fundamental than that already quoted. This other law is that all experience tends to evoke and progressively clarify an awareness of its own nature[1]. At the earliest (perhaps pre-natal) stages, then, this awareness must as yet have been extremely unclear; the person's sensations would not have been distinguished from each other, but on the contrary indistinguishable. Not till later would such a chaos have evolved into a cognitive field wherein any distinct portion stood out against a background still steeped in obscurity.

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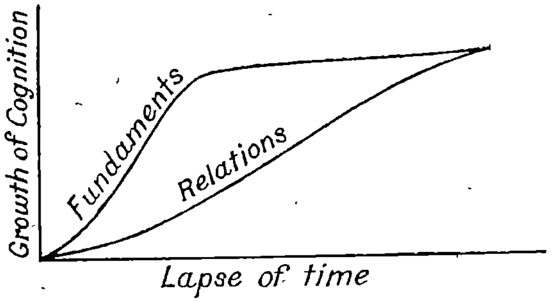
[1] *Nat. of Int.* ch. IV.

Apparently, the two laws in their working have some degree of co-operation. The earliest awareness of sensation may be accompanied by, or even depend upon, some degree of awareness of relation also. But certainly even after the awareness of sensation has fully developed, that of relations still long continues developing, as symbolised below.



Yet another law is relevant to our present purpose; this time it is one of quantitative nature. Its purport is that cognitive events by occurring tend to occur with greater ease[1]. From this law we may infer that the above described successive development in the perception of relatedness, however slow originally, would in course of time arrive at being effected rapidly. With adult persons, the most familiar portion of it would occur at too great a rate for the successiveness to be ordinarily detectable at all. In order to bring about detection, special experimental conditions become necessary[2]. The seeming unitariness in the operation of perceiving shapes would seem in this way to be satisfactorily explained. The growth of the perception of a tonal shape may truly enough, then, be compared to the building of a house out of bricks; but with the previso, that the bricks themselves are evolved out of aboriginally structureless clay. In the case of spatial perception, the clay never turns into separate bricks at all, but rather it cakes into a plaster.

One rather trivial misunderstanding remains to be dissipated. According to the description just given, the perception of shapes has its growth mainly through increment of more numerous and more clearly perceived relations. But the growth has also been described as being a transformation from one shape or whole to another. Now, this second mode of description, so far as it goes, is perfectly correct. But some writers—almost asking for trouble—have regarded the two modes of description as being in conflict.

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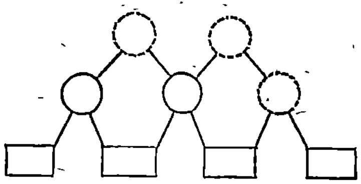
[1] *Nat. of Int.* p. 133.

[2] *Ibidem*, p. 161.

The actual facts may be symbolised in the following figure, where the continuous lines represent what is perceived at an earlier stage of development, whereas the dotted lines are the later increment of knowledge. If we talk of the growth in terms of relations, we are comparing the continuous part of the figure with the dotted part; it is like saying that a previously five-foot child has grown another six inches. Whereas if we adopt the terminology of shapes or wholes, we are comparing the continuous part with the entire figure; it is as if the previously five-foot child were said to have grown to five foot six. Either way of comparison is just as legitimate as the other. Each has its own appropriate employment.



IX. *Conclusion.*

On getting down to the really basal question, then, the warring unitarian and binarian schools appear to have only been looking at different aspects of the truth. These two aspects have here been reconciled by comprehension within the all-inclusive sphere of the ultimate cognitive laws, which yet a third school has been developing independently. Through such comprehension, the work which we have been discussing—some of it extraordinarily fine—acquires scientific foundation, whilst correspondingly this foundation becomes enriched with new superstructure[1].

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[1] The complete system of laws comprises, besides the two chiefly mentioned above, a third of qualitative nature, and five that are only quantitative.’ *Nat. of Int.* pp. 29–32.

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ECONOMY IN MOTOR LEARNING

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I. PREVIOUS INVESTIGATION OF THE SUBJECT.

IN the investigation of the most economical methods of learning, two problems have been dominant. The first is to compare the advantages of learning by wholes and by parts. In the whole method, the material is learnt by going through it completely time after time; in the part method, the material is divided into portions, and each of these is learnt before transition is made to the following one. G. E. Müller, under whose direction this problem was first investigated, found a marked superiority in the whole method[1]. This was subsequently confirmed by others, especially by Lakeman[2] and Pyle[3]. Both of these report that the whole method increases in relative effectiveness when the length of the material increases.

The second great problem—also primarily investigated under the direction of Müller[4]—and even before him by Ebbinghaus[5]—concerns the best way of arranging repetitions. A comparison was made between the plan of massing these all at one sitting and that of distributing them over several sittings separated by more or less lengthy intervals. Here, the advantage was found to lie with the distributive method. Moreover, this advantage was much greater in amount and much more unanimously confirmed by other investigators than in the case of learning by wholes.

The inquiry entered upon a new phase with the notable work of Pechstein[6]. The chief reason previously alleged for the inferiority of the part method was the difficulty in forming backward associations. Pechstein claimed to have overcome this difficulty by what he called the 'progressive' part method. Here, the first part is learnt completely; then the second part; then the first and second parts are learned together; then, the third part separately; then the three parts together; and so on.

His first experiments dealt with maze running. Both men and rats were used as subjects, and in each case, whether for men or for rats the progressive method showed a considerable advantage over the others, alike as regards the number of errors made, and the time occupied in learning.

Subsequently Pechstein worked with the learning of meaningless syllables, and obtained the results of exactly the same general character. His main conclusion, naturally, is that the progressive method is in general superior to all others. But he also found for the first time that massed repetitions are superior to distributed repetitions. This, he maintained, holds good in regard to *short* maze runs generally.

II. PRESENT EXPERIMENTAL SERIES A.

1. *Procedure.*

The learning consisted in mirror drawing and the same general method was adopted as has been already described in this *Journal*[7].

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[1] L. Steffens, *Ztsch. f. Psychol.* 1900, XXII.

[2] *Journ. Ed. Psych.* 1913.

[3] *Journ. Ed. Psych.* 1911.

[4] Jost, *Ztsch. f. Psychol.* 1897.

[5] *Über das Gedächtnis*, 1885.

[6] *Psych. Monogr.* 1917, XXIII; *Journ. Ed. Psych.* 1918, IX.

[7] 1923, Vol. XIV.

It remains only to explain how the task was divided into parts and how the repetitions were massed or distributed.

(*a*) *The whole method.*

Here the subjects were given three runs a day upon the whole of a star pattern to be traced out when only seen in a mirror. Between the runs there was an interval of about 30 mins. The learning was continued every weekday until the subject attained sufficient mastery to go round the pattern of 24 points twice consecutively within 28 strokes each. This latitude of four errors was allowed in order to shorten the time of learning, which would otherwise have been inconveniently long.

(*b*) *The pure part method* (*massed*).

The pattern was divided into quarters of six lines each. As in every quarter there are four diagonal lines and two vertical ones, they were presumed to be of sufficiently equal difficulty. The subjects (different from those for the whole method) were practised on the first quarter at the rate of 12 runs a day until they had mastered it. Ability to do two successive runs within seven strokes each (the minimum possible being six) was fixed as the standard to be attained. When the first quarter had been learnt in this way, the second, the third, and the fourth were learnt successively in like manner. Then the subject was given three runs a day on the whole pattern, until he was able to do two consecutive runs within 28 strokes each.

(*c*) *The progressive part method* (*distributed*).

The subjects (again different) were allowed three runs a day irrespective of the length of task. They first learnt the first quarter; then the second; then the two together; then the third; then the three together; then the fourth; and finally all four together.

(*d*) *Two part method.*

The subjects (once more different) were practised on the two halves at the rate of six runs a day. When these had been mastered separately, three runs on the whole were given until the learning was completed.

(2) *Results.*

(*a*) *Reliability coefficients.*

These were calculated from the scores for the learning of the first quarter under distributed conditions. Two series of ranks were obtained as follows. In the quarter there are six lines to be traversed. The scores for the first, third, and fifth lines were added together, and the subjects ranked accordingly. The other ranking was got in a similar way from the second, fourth, and sixth lines. The coefficient proved to be very high: ·95 by Spearman's Footrule formula converted so as to become comparable with the ordinary coefficient r (see Whipple's *Manual of Mental Tests*, p. 44).

(*b*) *Scores for the different subjects for the whole pattern.*

	Whole method	Pure part method (massed)	Progressive part method (distributed)	Two part method
	497	371	515	462
	552	485	622	625
	637	557	637	673
	714	593	650	807
	788	599	675	841
	791	760	693	1023
	801	834	727	—
	873	895	820	—
	892	967	908	—
	926	992	—	—
	—	1033	—	—
Median	790	760	675	749
Arith. mean	747	735	694	738

These data, so far as they go, seem to indicate the following conclusions:

(1) A considerable superiority of the progressive part method (distributed).

(2) Greatest inferiority of the whole method, and then of the pure part method (massed).

(3) A tendency of the pure part method (massed) to increase the scatter, as if the method were advantageous for some subjects but disadvantageous for others.

(*c*) *Scores for the different subjects for the first half of the pattern.*

By considering the results for the first half alone, all the procedure that is peculiar to the progressive method become eliminated. In this way, a comparison becomes possible between the scores for massing and distributing.

	Whole method, 6 runs a day	Part method, massed, 12 runs a day	Part method, distributed, 3 runs a day
	233	182	261
	375	227	328
	384	310	345
	452	362	368
	532	386	372
	639	397	412
	660	442	455
	—	468	461
	—	479	480
	—	500	527
Median	452	370	392
Arith. mean	468	375	401

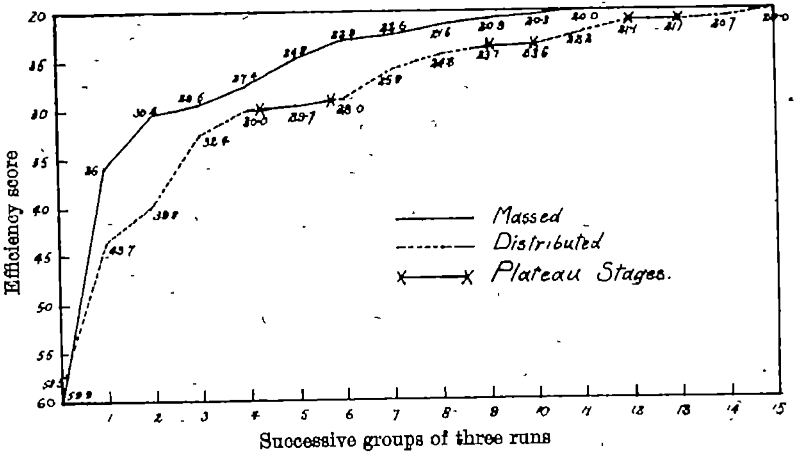
Here again the part methods, whether massed or distributed, seem to be more advantageous than the whole method. But between the massed and the distributed part methods, no significant difference appears. Indeed, if anything, the massing succeeds the better of the two.

(*d*) *Scores for the different subjects for the first quarter of the pattern.*

	Massed, 12 runs a day	Distributed, 3 runs a day
	77	173
	95	195
	130	202
	229	234
	229	284
	251	296
	254	355
	259	386
	329	390
	366	441
Median	240	290
Arith. mean	222	296

Here a decided advantage appears on the side of the massing. Moreover this advantage maintains itself throughout all the stages of practice, as shown in the following curve.

Learning Curves for First Quarter. (Mirror Drawing).



III. Present Experimental Series B.

(1) *Procedure.*

The object of this series was to test the effect of massing upon still shorter pieces of work. The subjects (again different) were first made to learn separately the four diagonal movements involved in the pattern, namely, expressed in terms of the compass, S.W. to N.E., S.E. to N.W., N.E. to S.W., and N.W. to S.E. Each of these diagonal movements contained the same number of lines (*i.e.* six) as the quarters of the pattern; in other words, the subject had to get from one mark to a second, then onwards in the same direction to a third mark, and so repeatedly up to a seventh mark. Thus, learning a diagonal movement amounted to massing repetitions on a very short piece of the pattern. One group of subjects was given 12 runs a day on the diagonal lines, each in succession until it was learnt. As before, ability to trace the six lines in not more than seven strokes twice consecutively was taken as the standard of learning to be attained. Finally, three runs were given on the whole pattern. Another group of subjects acted similarly, but had only three runs a day on the diagonals.

(2) *Resulting scores.*

(*a*) *The diagonals.*

	Massed, 12 runs	Distributed, 3 runs
	269	265
	540	399
	695	408
	760	426
	766	444
	828	456
	840	479
	—	511
Arith. mean	671	431

Evidently the massing has become very disadvantageous indeed.

(*b*) *The final runs on the whole pattern.*

Here again the massing showed at a great disadvantage, the arithmetic mean being 355, as against 266 for the distributing.

IV. CONCLUSIONS.

(1) *Ambiguity of 'massing' and 'distributing.'*

The repeating of a task is said to be 'massed' when no interval is given between the repetitions; it is 'distributed' when, on the contrary, intervals do occur. Suppose, for instance, the task was done four times, there would be complete massing and no distribution if all four repetitions were done at a single sitting. There would be some degree of massing and also of partial distribution, if two were done at one sitting and two at another. There would be no massing and complete distribution, if all four were done at different sittings.

So far all is straightforward enough. But ambiguity is introduced when the possibility is contemplated of the parts being done in other than their natural order. For then the massing, as also *per contra* the distribution, may be of at least three different kinds.

To illustrate this, let us suppose the task to have the two parts A and B; and let there occur two performances of each, as A_1, A_2, B_1 and B_2. If the work done at each sitting is depicted by a single vertical series of the letters, there arise six main types of arrangement for the whole work.

I	II	III	IV	V	VI
A_1	A_1	$A_1\ B_1$	$A_1\ A_2$	$A_1\ B_1\ A_2\ B_2$	$A_1\ A_2\ B_1\ B_2$
A_2	B_1	$A_1\ B_2$	$B_1\ B_2$		
B_1	A_2				
B_2	B_2				

Any of these arrangements can be expressed in terms, either of massing, or of distribution.

Thus, arrangement I has

(*a*) complete *general* massing as between sittings, in that all four pieces of work are done at one sitting;

(*b*) complete *part* massing as between sittings, in that both A's, as also both B's, are done at one sitting;

(*c*) part massing within the sitting, in that both A's, as also both B's, are consecutive.

Expressed in terms of distribution instead of massing, arrangement I has,

(*a*) no general distribution as between sittings;

(*b*) no part distribution as between sittings;

(*c*) no distribution within the sitting.

Turning to the other arrangements, II possesses (*a*) and (*b*) but not (*c*). III has (*b*) and (*c*), also (*a*) in some degree; it is the type followed by the present experiments with 'massing.' IV has (*a*) in some degree, but neither (*b*) nor (*c*); it is the type of the present experiments 'distributed.' V and VI have neither (*a*) nor (*b*) nor (*c*).

An instance of how confusion can arise may be taken from certain results given by Pechstein.

Method	No. of trials	Time	Errors
'Part, distributed'	23	1220″	237
'Whole, distributed'	12	641	126

Now, any reader of a table so expressed might easily gather that, under equal conditions of distribution, the whole method had shown a great advantage. But the work was really done with the following arrangement (where again each vertical series of letters represents a single sitting):

A. Part method, 'distributed.'

A_1 A_4 A_7 A_{10} A_{13} ...
A_2 A_5 A_8 A_{11} A_{14} ...
A_3 A_6 A_9 A_{12} A_{15} ...

and so on until A is learned. Then similarly with B, C and D. Finally, until the whole is learned, as follows:

A_1 A_4 ...
B_1 B_4 ...
C_1 C_4 ...
D_1 D_4 ...
A_2 A_5 ...
B_2 B_5 ...
C_2 C_5 ...
D_2 D_5 ...
A_3 A_6 ...
B_3 B_6 ...
C_3 C_6 ...
D_3 D_6

B. Whole method, 'distributed.'

A_1 A_4 A_7 ...
B_1 B_4 B_7 ...
C_1 C_4 C_7 ...
D_1 D_4 D_7 ...
A_2 A_5 A_8 ...
B_2 B_5 B_8 ...
C_2 C_5 C_8 ...
D_2 D_5 D_8 ...
A_3 A_6 A_9 ...
B_3 B_6 B_9 ...
C_3 C_6 C_9 ...
D_3 D_6 D_9 ...

and so on till completion of the learning.

Evidently the two procedures do indeed give the same amount of *part* massing by sittings (and consequently of part distributing also). But they are widely apart as regards *general* massing. Note in particular how extremely small this is in the so-called part method except just at the end. And to this latter difference, much more likely than to the whole method as such, the advantage may be ascribed.

(2) *Factors in success.*

We may consider these by taking in turn the various ways in which the effect of general or part massing by sittings differs from that of repetitions distributed in different sittings.

(*a*) *General fatigue.*

General massing by sittings must tend to produce general fatigue; and this would be manifested, both in the performance at the time, as also in the accuracy of remembrance afterwards. But according to the present evidence[1], this influence might be expected to be small.

(*b*) *Specific fatigue.*

This would be derived from part massing as between the sittings, and enhanced by part massing within the sitting. A notable instance of what is meant by such specific fatigue can be found in the phenomenon of reversible perspective[2]. It would be proportional to the monotony of the task. It might become strongly influential.

(*c*) *False associative connexions.*

These would derive solely from part massing within the sitting.

(*d*) *Consolidation.*

As G. E. Müller has strongly insisted, the recall of anything memorised is injuriously affected by any immediately subsequent further memorising. This would engender harmful results from all general massing, which might be slightly enhanced by complication with the other two kinds of massing.

(3) *Noegenetic work*[3].

(*a*) *Advantage to be expected from massing.*

All the factors so far mentioned would apply to memorising as well as to noegenetic work, but (*c*) only to memorising; they would all be against massing and in favour of distributing. Small wonder, then, that throughout previous research—which had been devoted to memorising[4]—the massing had been found to do harm. Let us now consider the effect of the different arrangements upon noegenetic work, as elucidated in a previous publication[3]. We found that usually two stages were involved. In the first place, relations are educed both between different items in the cognitive field, as also between these on the one hand and the subject's own movements and intentions on the other. Secondly, these relations are recalled to mind and applied both to similar items and even to different ones, in such a way as to educe the needful correlative movements. Now, for *this* purpose, evidently part massing, and even general massing, may be very advantageous. For the said recall of the previously perceived relations will occur much more readily within the same sitting.

(*b*) *Empirical corroboration.*

The preceding theoretical consideration is strongly corroborated by the unexpected result of Pechstein, who—contrary to all previous investigators—found that massing was, for short pieces of work, of marked advantage. For indubitably, the learning of the maze is no mere memorising, but mainly a noegenetic task. Further corroboration is furnished by the present experiments. We have found evidence that massing is slightly advantageous for working on a half of the star, and very decidedly advantagous for working on a quarter of it.

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[1] Phillips,

[2] Flügel, This *Journal*, 1913, v, pp. 357 ff.

[3] Spearman, *The Nature of Intelligence*, 1923.

[4] Especially, the establishing of associative connections in meaningless material.

(c) *The case of longer spells of work.*

But the question arises why, in the case of longer spells of work, the advantage left the massing method and went over to that of distribution. The explanation appears to be simple. What is here called the method of 'massing' refers only to part massing. As regards the general massing, the two methods were on equal footing.

Now part massing engenders the noegenetic applying of relations from one line to others within the same part. But massing which is of the general but not part kind engenders the applying of the relations to *other* parts, and thus extends the scope of the advantage. Until serious fatigue sets in, then, it would appear that noegenetic work—quite contrary to memorising—is benefited by all massing, but by the general kind even more than by the part kind alone (so long as the parts are sufficiently alike to allow of the relations being applied from one to another).

(d) *The case of very short spells of work.*

The further question, as to why the massing ceased to be advantageous in the case of very short runs, is already answered. The massing was exclusively of the part kind. Very likely, however, a considerable amount of influence must in this case be ascribed to a further factor, specific fatigue. This is indicated by the findings of our previous communication[1].

(4) *Advantages of the progressive method.*

The superiority claimed for this method by Pechstein has, so far as the present results go, been corroborated. But a few words may be allowed as to its source. Pechstein himself says that its advantages "must depend mainly on the degree to which place associations are obviated." But in the present experiments, no place associations seem to be influential. Moreover, even his own data show that the advantages of the progressive over the pure part method are noticeable only in the case of distributed repetitions, not in that of massed ones.

There would appear to be another main cause of the effectiveness of the progressive method. Its procedure, we should remember, is just the same as that of the other method with respect to the learning of each part separately. Accordingly, our present experiments found no advantage here; the average score for learning the four quarters separately for the pure part method was 415, and for the progressive method 466. The connecting of the quarters with each other is where the progressive method becomes superior, its score being only 228, as against 314 by the pure part method. Now, in the case of the pure part method, this inter-connecting of the parts starts abruptly, at the end of the work, with all four quarters. Whereas, in the progressive method, it first connects two of them, then three, and finally four. In such a gradual lengthening, we venture to suggest, the main superiority of the progressive method really lies. That is to say, it is the best means of securing the most favourable length of learning material suitable for each stage of habituation.

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[1] This *Journal*, xiv. 274.

(5) *General law for noegenetic work.*

If from the scanty results so far gleaned, an attempt be made to formulate a general law about the most economic method for noegenetic work of such kind as maze learning or mirror drawing, it might run somewhat as follows. *Massing is advantageous up to the limits where fatigue, specific or general, begins to be seriously felt. If the parts are such, that relations learned in one can be applied to another, then general massing is better than purely part massing.*

Be this as it may, we certainly seem entitled to draw the conclusion that the results about massing and distribution at present holding the field, and drawn almost exclusively from the investigation of memorising work, required thorough revision for the still more important work of a noegenetic kind.

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NOTE. As this and the previous papers as originally submitted by me to the editors of this journal were considered too long, I gratefully record that I owe it to Prof. Spearman for generously consenting to give much of his valuable time towards condensing the papers. And as would be expected they now bear the indelible stamp of Dr Spearman in their keenness of analysis and terseness of expression. My thanks are also due to Mr. Perera for invaluable suggestions, to Mrs Cooks for ready collaboration, and to Headmasters Linecar and Millward for kindly providing the necessary facilities for conducting the research.

CONCERNING THE USE OF THE PSYCHO-GALVANIC REFLEX IN PSYCHOLOGICAL EXPERIMENTS

By E. FARMER and E. G. CHAMBERS.

(1) *Introduction* (p. 237).
(2) *Résumé of past work* (pp. 236–246).
(3) *The psycho-galvanic reflex and diurnal rhythm* (pp. 246–248).
(4) *Some points of technique* (pp. 248–250).
(5) *Physical and physiological factors underlying the psycho-galvanic reflex* (p. 250).
(6) *Conclusion* (pp. 250–251).
Bibliography (pp. 251–254).

(1) INTRODUCTION.

It has long been known that the human body manifests galvanic phenomena, by which is meant that when a subject is attached to a galvanometer and appropriate stimuli are given, the galvanometer is deflected and permits us to register the amount and direction of the deflexion. Although there can be no doubt about the existence of the phenomenon, yet there is considerable doubt as to its interpretation. The difficulty of interpretation is mainly due to the interference of factors which are at present uncontrolled, and until we know something more of their effect the very greatest caution must be exercised in the interpretation of the psycho-galvanic reflex.

During the course of an investigation carried out on behalf of the Industrial Fatigue Research Board we had occasion to use the psycho-galvanic reflex as a means of group differentiation. The actual results of the investigation are being withheld from publication pending further research, but certain points of technical interest came to light which we thought would be helpful to other workers in the same field, and by making these public we hope we may save others unnecessary labour. We shall confine ourselves therefore, to a résumé of past work and a statement of facts and shall refrain as far as possible from their interpretation.

(2) RÉSUMÉ OF PAST WORK.

Experimenters who have dealt with the subject can be divided into two main groups, (1) those who passed an external current through the body of the subject, and (2) those who utilised the electro-motive force of the subject's body. Féré, Veraguth, Jung, Binswanger, Radecki, Waller, Prideaux, and Whately-Smith are among the former class; and Scripture, Menzerath, Tarchanoff and Sticker are among the latter. Some experimenters, *e.g.* Peterson and Sidis have used both.

Apparatus.

Although galvanometric deflexions are obtained when the body is connected with the galvanometer whether an external current is used or not; it is obvious that the phenomena are not due to the same immediate cause in both cases. When an external current is passed through the body deflexions of the galvanometer indicate a change in apparent bodily resistance to this external current; when, however, no external current is used these deflexions are due to changes in the electro-motive force generated in the body itself. Sidis and Nelson (46), Veraguth (52) and Tarchanoff (50) all report the gradual diminution of the galvanic reaction to the constant repetition of stimuli. This may be due to a gradual lessening of sensitivity, but when no external current is used it may at least in part be due to diminution in internally generated electro-motive force.

Experimenters differ considerably as to what they regard as the most satisfactory kind of electrodes. Fürstenau (8) held that it was better to use electrodes of different material and so get the current going in one direction, which causes the galvanometer deflexions to be greater and so easier to measure. Gregor and Gorn (15) originally used zinc and carbon electrodes, but they found that these allowed the sweat glands to play too large a part in the reaction and that liquid electrodes removed difficulties due to sweat and movements of the subject. Müller (31) used nickel, Sidis and Kalmus (45) and Guilleminot (18) liquid, and Starch (48) Ostwald's normal calomel electrodes. Prideaux (39) discarded the use of liquid electrodes because he found the size of the subject's hands and feet altered the area of contact with the electrodes and so altered the resistance. Gregor and Loëwe (16) found that resistance is lowered by using damp electrodes, and they infer that the size of the galvanic reaction depends on the degree of dampness in the skin. With dry electrodes the change of resistance is greater, but the galvanic reflex is small in people with insufficient sweat glands. The advantage of metallic electrodes is that in theory the area of contact remains constant, although in practice it may not, because by slight movements in the hand of the subject the area of contact may alter. Prideaux's method of using zinc electrodes covered with wash-leather soaked in saline solution obviates this difficulty, and ensures a constant area of contact.

The usual procedure after having attached the subject to the galvanometer is to administer certain stimuli (such as loud noises, pin-pricks, threats of injury, etc.) calculated to disturb the subject's nervous sensitivity. Although the effects of this differ both according to the subjects and to the nature of the stimuli given, yet all experimenters agree that such stimuli are followed by galvanometric deflexions, which, when an external current is used, show a lessening in the subject's apparent resistance, *i.e.* an increase in conductivity, and when no external current is used, a change in potential, resulting in increased electro-motive force. Any stimulus which sets up an emotional activity in a normal subject produces deflexions of the kind described, and therefore the phenomenon has come to be called the *psycho-galvanic reflex*.

Latent period.

A feature of these deflexions which has been noted by many experimenters, among whom are Veraguth, Peterson, Scripture, Herman, Lucksinger, Binswanger, Jung, Sidis, Kalmus, Abbott and Wells, and Prideaux, is a *latent period* between the giving of the stimulus and the galvanic response. This latent period is different from the ordinary reaction time between the giving of a stimulus and the response, being much longer, usually in normal subjects about 2·5 seconds, and in abnormal subjects being as much as 20 seconds (33). This latent period has come to be looked upon as the sign of a genuine psycho-galvanic reflex as opposed to the deflexions due to a change of contact, and since it is known to vary with different people and with the same person at different times (Peterson and Jung (34) and Veraguth (52)) further research on this point alone might yield very fruitful results. Herman and Lucksinger (23) have already noted that the latent period is shorter in warm-blooded than in cold-blooded animals; and Abbott and Wells (1) report that the latent period is normal in stuporose conditions. Prideaux (39) thinks that this latent period is evidence against the James-Lange theory of emotion.

Curve of Physical Accommodation.

Another phenomenon which has been noted by experimenters is that the resistance of the subject tends to diminish rapidly during the first part of the experiment. Guilleminot (18) regards this as due to *ionisation* in the skin and that the diminution continues as long as the ionisation is incomplete; so that all measures of the psycho-galvanic reflex which do not take this factor into account are inaccurate. Veraguth (51) found that it took about 15 minutes before the psycho-galvanic curve reached stability. The speed of this process depends both on the concentration of the electrolyte used and also on the nature of the electrolyte itself, *i.e.* on the ions used. Vigouroux (55) measured the resistance of several persons on every day for a month one minute after they had been attached to the galvanometer; he found that each person's resistance was constant. Assuming that he used the same electrolyte at the same concentration this would seem a reliable way of measuring resistance while paying no attention to deviations in resistance.

Objective Strength and Stimuli.

Starch (48) and Veraguth (51) say that the psycho-galvanic reflex corresponds roughly with the strength of the stimuli given, but Philippson and Menzerath (36) point out that there is a closer correspondence between the subjective states caused by the stimuli and the corresponding deflexions than between the objective strength of the stimuli themselves and the corresponding deflexions. Since we have already seen that repeated stimuli lose their effect, causing no deflexion at all in some cases, this view would seem to be correct; and it is important as pointing to the subjective nature of the psycho-galvanic reflex.

Respiration, etc.

Besides the deflexions due to the stimuli mentioned above, deflexions are also caused by coughing, sighing and deep breaths. Peterson and Jung (34), Philippson and Menzerath (36), and Tarchanoff (50) have experimented on the connexion between respiration and the psycho-galvanic reflex but found no definite connexion. Peterson and Jung (34), however, report that when the psycho-galvanic and respiration curves are disturbed the former regains equilibrium more quickly than the latter. Veraguth found that there is no parallelism between pulse rate, respiration, reaction time in association experiments, and the psycho-galvanic reflex.

Muscular movements.

Moravcsik (29) reports that muscular movements do not affect the psycho-galvanic reflex if the electrodes are not affected, although Du Bois-Reymond (5) first called attention to changes in electro-motive force generated by the contraction of a limb. There can be little doubt that the contraction of a limb changes the internal electro-motive force, but it is possible that when an external current is used muscular movement does not affect the psycho-galvanic reflex except in so far as it causes changes in affective tone and electro-motive force.

Curve of Expectancy.

Peterson and Jung (34), Veraguth (52), Sommer (47), and Fürstenau (8) among others, have noted what they call a *curve of expectancy*, by which they mean that resistance tends to diminish gradually during the state of general nervous tension set up in the subject in anticipation of some unknown stimulus. As this anticipation curve is more likely to occur at the beginning of the experiment before the subject has become accustomed to his surroundings, the greatest care must be exercised in distinguishing it from the gradual diminution in resistance due to physical accommodation. In practice it is not difficult to recognise the difference when examining photographic records, for the curve of physical accommodation is continuous, whereas expectancy curves are alterations in the accommodation curve. The presence of these two curves does, however, immensely complicate any mathematical method of treating data.

Intellectual effort.

The relation between intellectual effort and the psycho-galvanic reflex has been investigated by some experimenters. Veraguth (52) found that the reading of an uninteresting newspaper article gave very little rise in conductivity while the reading of an interesting article gave a large rise. Philippson and Menzerath (36) found that all mental work gave psycho-galvanic deflexions, but these were not so large as those given by emotions, and they accordingly conclude that the size of the deflexions caused by intellectual work is dependent upon the effort put forth by the subject; and with this Moravcsik (29) is in agreement. This effort probably has the effect of disturbing emotional stability and by this means indirectly affects the psycho-galvanic reflex. Miss M. D. Waller (62) found sensitiveness to the psycho-galvanic reflex to be associated with intellectual efficiency as measured by the class order of 73 women medical students. Gregor and Gorn (15), however, found no correlation between intelligence and the psycho-galvanic reflex. Before we can come to a conclusion on this matter, it will be necessary to await the results of further experimental work.

Memory and Association Tests.

Memory also affects the psycho-galvanic reflex. Veraguth (52) found that self-given memory stimuli, *i.e.* the recalling to memory of situations which at the time of their happening had strong affective tone, had the effect of lessening resistance. Philippson and Menzerath (36) also report the same phenomenon. Veraguth and Coëtta (53) among others, experimented on the relation between the psycho-galvanic reflex and the reaction time in word association, and they found that on the whole long reaction times corresponded to large psycho-galvanic deflexions. In an experiment consisting of 70 association words they found that the best method of measuring affective tone in the first half was by means of the psycho-galvanic reflex, but that in the second half of the experiment reaction time was the more reliable indicator, on account of the gradual diminution of the psycho-galvanic response. Peterson and Jung (34) found that the psycho-galvanic reflex and reaction time were in some cases parallel and in others not. They think that the psycho-galvanic reflex only shows the effect of acute affective states and not of long drawn-out affects. Binswanger (4) found no connexion between the psycho-galvanic reflex and reaction time in association experiments. He explains this by saying that a long reaction time may be caused by the subject failing to understand the stimulus word. When this is the case no correspondingly large psycho-galvanic reflex will take place because the subject's failure to understand the meaning of the stimulus word will deprive the latter of any affective associations. Veraguth (52) found no parallelism between the psycho-galvanic reflex and reaction time in association experiments, although he found that where a complex was known to exist the psycho-galvanic reflex connected with the complex words was high. Peterson and Jung (34) found that altered reproductions in association tests, which they regarded as indicating complexes, gave psycho-galvanic deflexions 2 mm. in excess of the average of ordinary reactions in four subjects whom they examined.

Results obtained from Pathological Subjects.

Veraguth (52) found that patients suffering from local anaesthesia gave no response when the affected member was stimulated, although the patients could see the stimulus being given. Hysterical patients showed disproportion between their objective and subjective reactions and he regards the psycho-galvanic reflex as a means of distinguishing between organic and hysterical anaesthesia. Prideaux (39) also thinks that the psycho-galvanic reflex tends to show that the emotions of hysterics are probably artificial. Vigoroux (55) found that hysterical anaesthetics and all hysterics have higher resistances than normal subjects, and this same high resistance was found in subjects suffering from spinal paralysis when the disease was not of too long standing; but when the disease had been present for a long time the resistance was diminished. He found that resistance was diminished in 'Basedow's Disease,' and quotes La Seta and Silva as agreeing with him. Peterson and Jung (34) found that patients suffering from hebephrenia did not differ from normal subjects in the type of psycho-galvanic reflex they manifested. In katatonic forms of dementia praecox the latent period was longer than with normal subjects, and the psycho-galvanic reflex was gradual in appearance and very small if present at all. In chronic katatonia only the severest stimuli had effect, and patients suffering from acute katatonia gave no response at all. Gregor and Gorn (15) in treating patients with katatonia, manic depressive insanity, progressive paralysis, and psychasthenia found that as they became better their psycho-galvanic reactions gradually became normal. They also found that congenital idiots had very large psycho-galvanic reactions but of the normal type. A border-line patient showed a normal reaction, while three patients suffering from a mild form of imbecility showed small reactions. Prideaux (39), however, found that out of 17 idiots he examined, 12 gave no reactions at all, although they perceived the stimulus. Gregor and Gorn (15) found that epileptics gave uncertain results. Senile dements, whose intellectual processes were affected but who were otherwise normal, gave normal psycho-galvanic reflexes. No psycho-galvanic reflex was found in patients suffering from arteriosclerosis. Prideaux (39) found a difference between the reactions given by conversion hysterics and anxiety hysterics, the former giving small reactions and the latter large ones; and that patients suffering from cortical degeneration or mal-development gave either no response at all or very small ones, while in hypothyroidism, the psycho-galvanic reflex is increased in size.

Müller (31) injected a 5 per cent. solution of novocain and adrenalin which had the effect of entirely destroying the psycho-galvanic reflex. When the nerves leading to the palms of the hands or the soles of the feet where the electrodes were attached were severed, no reaction took place, but after some days the backs of the hands and the feet, which had previously given no reaction, manifested the phenomenon. This may be due to nerve transference. Whateley-Smith (64) found that the psycho-galvanic reflex was diminished by the use of alcohol, whereas

Waller (59) found that alcohol did not affect the reflex, though chloroform and morphia lowered it. Tarchanoff (50) also notes that physically and mentally tired people give practically no psycho-galvanic reflex, and Prideaux (39) states that men are more constant than women, and that the reflex is diminished by fatigue and menstruation, Binswanger (4) says that the resistance is heightened during sleep.

Action of Sweat Glands.

Leva (27) explored the whole surface of the body with a view to determining the correlation between the number of sweat glands in the skin at any part of the body and the size of the psycho-galvanic reflex when the terminals were attached to that part of the body, and he found a very close correlation between these two. Peterson and Scripture (35) thought the psycho-galvanic reflex due to the action of the sweat glands, and found that it could be altered by saturation. Sidis and Kalmus (45) covered the whole of the hand with shellac and paraffin and found that the psycho-galvanic reflex was not altered; but as Peterson and Scripture (35) say this experiment must have been defective, because if the hand had been properly covered the current would have been cut down to zero. Leva (27), Herman and Lucksinger (23) and Waller (58, 59) injected atropine in order to prevent sweating. The first two found that the psycho-galvanic reflex disappeared whilst Waller found that it was unaffected. Mrs Markbreiter (28), working in Waller's laboratory, came to the same conclusion as he did with regard to the effect of atropine, and explained Leva's result by saying that since atropine by stopping sweating raised the resistance, deflexions are made more difficult to detect, so that Leva, not paying attention to this fact, may have overlooked them if his galvanometer was not sufficiently sensitive. Peterson and Jung (34) found that formalin did not prevent the psycho-galvanic reflex, and Waller (59) found that a subcutaneous injection of ·005 grm. of pilocarpin hydrochloride to augment sweating did not alter the psycho-galvanic reflex. Vigoroux (55), Binswanger (4), and Tarchanoff (50) regard the action of the sweat glands as the primary cause of the psycho-galvanic reflex. The last explains this by saying that the sweat glands act as soon as the nerves have done so, in order to cool the body after the heat set up by the nervous strain. If this is so, it would to some extent explain the latent period mentioned above which takes place between the experiencing of an emotion and its corresponding psycho-galvanic reflex. In the face, however, of the conflicting results reported by various experimenters it would be unwise to draw any definite conclusion as to the part played by the sweat glands in the psycho-galvanic reflex. Waller(59), in order to overcome these contradictions, suggests that the relation between the psycho-galvanic reflex and the sweat glands may not necessarily be a serial one, *i.e.* one causing the other, but a parallel one.

Capillary action.

Sticker(49) and Radecki(41) think the psycho-galvanic reflex to be due to changes in capillary circulation. Waller(58) and Peterson and Jung(34) rendered the hand pulseless and exsanguine by means of a rubber band but found that the psycho-galvanic reflex was not appreciably affected, and the latter found no change when the veins were supercharged by artificial venus stasis. Leduc(26) experimented with liquid electrodes at different temperatures and found that resistance did not depend on vascularity. Prideaux(39) found that the psycho-galvanic reflex was increased during a condition of general vaso-dilatation though unaffected by the local cutting off of the blood supply. The conclusion to be drawn from these experiments seems somewhat similar to that suggested by Waller with regard to the action of the sweat glands, namely, that the relation between the phenomena is probably parallel and not serial. An emotional change affects the psycho-galvanic reflex and probably also the sweat glands and capillary system, but it does not follow that if the latter are put out of action at the periphery the psycho-galvanic reflex is abolished, though if they are affected centrally there may be a corresponding effect in the psycho-galvanic reflex. Sidis and Nelson(46) severed the nerves leading to the parts of the skin of animals attached to the galvanometer and found no alteration in the psycho-galvanic reflex. This result is in direct contradiction to the results reported by Müller (see p. 243). They had already satisfied themselves that the phenomenon was not due to changes in resistance, circulation, or secretory currents, and came to the conclusion that the phenomenon was entirely due to electro-motive force set up by muscular action. Prideaux(38) in his criticism of these experiments points out that their technique does not make it clear whether they were dealing with the psycho-galvanic reflex or with the effects of the current set up by muscular oscillations already pointed out by Du Bois-Reymond(5). Until this is made certain, in view of Müller's experiment and of the experiments already recorded made on patients suffering from cortical degeneration, we are entitled to regard the central nervous system as playing a large part in the psycho-galvanic reflex, although this does not exclude the possibility that its immediate cause is peripheral. Prideaux (39) found that the two hands attached to two different galvanometers gave different results; and Veraguth (52) found that when one galvanometer was attached to the hand and one to the foot at the same time, and constant stimulation had caused the psycho-galvanic reflex in the hand to disappear, deflexions could still be obtained if the galvanometer attached to the foot was put into circuit. Both of these experiments point to the immediate cause of the psycho-galvanic reflex as being peripheral.

Polarisation.

As has already been pointed out, in the absence of an external current the psycho-galvanic reflex is due to changes in internally generated electro-motive force, but when an external current is used the deflexions are due to changes in conductivity caused by changes in apparent resistance, although it is possible that electro-motive force still plays a part. Polarisation has been suggested as an explanation of this phenomenon. Gildemeister (13) passed an electric current through the body, and separately through the skin, of a dead frog and he found that the effects were the same; from which he concludes that polarisation takes place chiefly in the skin, though Sidis and Kalmus (46) found that the psycho-galvanic reflex was unaltered when the action of the skin was eliminated by using hypodermic needles as electrodes. They agree with Philippson and Menzerath that polarisation tends to disappear after a time, which accounts for the rise in conductivity. Binswanger (4) says that when using an external current we are dealing with polarisation and not with the action of the sweat glands, which he regards as the determining factor in the psycho-galvanic reflex. Prideaux (39), from some experiments on the physical nature of the reflex, came to the conclusion that it is a polarisation effect. By using alternating currents of 5000 per second frequency polarisation was abolished as was also the psycho-galvanic reflex; and he also found that by diminishing the current and thereby diminishing the polarisation, the psycho-galvanic reflex also diminished *pari passu.*

(3) THE PSYCHO-GALVANIC REFLEX AND DIURNAL RHYTHM.

We carried out an experiment in order to see how far daily rhythm, cheek and clinical temperature, kata-thermometer readings, pulse rate, the taking of food, and sleep affect the psycho-galvanic reflex. For this purpose three male subjects were attached to the galvanometer in turn, each for 25 consecutive hours so as to allow one hour for physical accommodation and afterwards be able to obtain a 24-hour galvanic curve.

It was found that:

1. The curve of physical accommodation continued for at least an hour after attachment to the galvanometer.

2. There was a drop in resistance following meals, and a rise in resistance preceding meals, and from 10 p.m. onwards after the effects of dinner had passed.

3. There was a very great rise in resistance during sleep. The rise was very rapid at the beginning of the night and rose to a peak immediately before getting up in the morning. The drop after being awakened at 7.30 a.m. was as rapid as the rise the preceding night.

4. There was no correspondence between the average resistance for the first hour when physical accommodation is taking place and the average resistance for the subsequent hours, nor was there any correspondence between the average resistance and the number of psycho-galvanic reflexes.

Before the results of the experiments were known to any of the subjects, each wrote out a careful self-analysis of his own character with special reference to the operation of the particular emotions as classified by McDougall. Such estimates of emotionality must necessarily be open to the criticism that they are based on self-judgment. Each of these subjects, however, was a trained psychologist accustomed to scientific introspection, and was far more interested in the results of the experiment than in the judgment of his colleagues (who alone saw the analyses) as to his moral qualities. This attitude of mind perhaps removed to a very large degree any error that may enter into this self-analysis of character through false shame or modesty, so that a considerable degree of reliance may be placed on these self-estimates of emotionality. These analyses showed an exact correspondence between the degree of emotionality as measured by the psycho-galvanic reflex and as measured subjectively. The subject who had the highest degree of emotionality estimated both subjectively and objectively, had the deepest sleep with no galvanic responses of any kind, his resistance during sleep being a perfectly straight line at a resistance of 32,000 ohms. The least emotional of the three subjects had the largest number of galvanic responses during sleep, and had a varying resistance, reaching its highest point at 9,400 ohms. The responses given during the night were apparently due to a dream which he remembered having had but the content of which he could not recollect. It was obvious to the experimenters who were watching him during sleep that his dream was an exciting one as he moved his head about considerably and also talked in his sleep. He alone of the three subjects noted that a good deal of conscious suppression took place during the waking hours, and it is possible that these suppressed conflicts solved themselves during sleep by means of dreams. Neither of the other subjects remember dreaming nor did the experimenters see any signs of their doing so as they watched them during sleep. One of the subjects was aroused during the night and so had his sleep broken, and his resistance at once fell to the normal level of his waking period; and also another subject fell asleep in the daytime and his resistance immediately rose; so that we can conclude that the high resistance during the night was definitely due to sleep and not to diurnal rhythm.

Clinical and cheek temperatures, kata-cooling power, and pulse rate were taken hourly during waking hours and the results correlated with resistance and the number of psycho-galvanic reflexes manifested during the observational periods, but the results were too inconsistent to warrant the conclusion that these factors have any direct effect upon the psycho-galvanic reflex.

All the subjects gave more reflexes during observational periods, meal times, and while entertaining visitors (ladies).

Changes in apparent resistances due to fatigue, and the recuperative effect of meals are too marked to make resistance alone a reliable measure of emotionality; this however does not apply to the psycho-galvanic reflexes which are deflexions in the curve of resistance.

(4) SOME FACTORS AFFECTING TECHNIQUE.

The following points came to light during an experiment on the psycho-galvanic reflex in the Cambridge Psychological Laboratory.

(1) A strong salt solution (25 per cent.) has the effect of reducing apparent resistance and quickening the process of physical accommodation more than the weaker ones used by most experimenters. When resistance, as opposed to changes in resistance, is taken as a differentiating measure the strength of salt solution used should always be stated.

(2) Increased voltage decreases apparent resistance. The voltage should therefore always be stated.

(3) Physical accommodation takes place with equal rapidity when the electrodes are attached to the hand whether the current is turned on or not, thus disproving that accommodation is due to ionisation.

(4) A gradual rise in apparent resistance was manifest at the end of

each working period, corresponding to the one before meals, noted in the experiment in connexion with diurnal rhythm. This may indicate a relation between apparent resistance, as opposed to deflexions in the curve of resistance and fatigue.

(5) It was found that the right hand had a greater resistance than the left in all subjects examined.

(6) Large electrodes were found to lower apparent resistance and so make all deflexions easier to read. When they were properly applied the larger electrodes were not more susceptible to changes of contact through movement than the smaller ones.

(7) A small hole was accidentally made in the skin of one of the subject's hands by a grain of undissolved salt. When the electrodes were afterwards applied to this hand it was found that its resistance was more than usually low, and that a voltage of more than 6 became so painful that it was impossible to continue the experiment. This would lend weight to the theory that resistance is chiefly in the skin.

(8) During the experiment it was found that those constantly employed as subjects failed after a time to give any deflexions even when the most severe stimuli were given. After a week end in the country the subjects again manifested the normal reflex. We have no data to show how far this failure in response was due to physiological or psychological causes.

(9) Parallel reflexes were found in both hands of the same subjects, caused simultaneously by the same stimuli. It may be possible by examining parallel reflexes to determine whether the reflex is absolute or dependent wholly or partly on resistance. We found considerable difficulty in getting absolutely comparable galvanometers, so that we do not feel justified in using our data to arrive at any definite conclusion in this matter.

(10) Parallel reflexes were given by the two hands, one of which was connected directly to the galvanometer without any external current, while the other was attached to the Wheatstone bridge and a current passed through it. Experiments along this line should manifest any differences in the reflex when it is due to a change in electro-motive force and when it is due to a change in apparent resistance, and also determine what part, if any, deflexions due to electro-motive force, play in those which are due to changes in apparent resistance.

(11) The reflex with latent period was caused by the vigorous movement of any limb of the subject except the one attached to the galvanometer. If this limb is moved a deflexion was caused without a latent period and this was shown to be due to change of contact.

(12) When the hand was connected first to an accumulator and directly to a galvanometer immediately afterwards by means of a two-way key, it was found that the galvanometer registered a large deflexion, showing that the hand had apparently been charged like a cell. This deflexion very quickly died away to zero and thus was clearly distinguishable from a deflexion due to internally generated electromotive force.

(13) It was found that there is a drop in potential across the contacts between the terminals and the hand, and that a voltmeter attached to these terminals registered deflexions simultaneously with the subjects' reflexes. How far these voltmeter deflexions are due to electromotive force and how far to changes in apparent resistance has yet to be determined.

(6) CONCLUSIONS.

From the above résumé of the work of others and from a consideration of some of the points of technical interest which have come to light in our own work it is clear that the problems in connexion with the psycho-galvanic reflex can be divided into three groups—the physical, the physiological and the psychological.

Physical problems.

(1) Is the psycho-galvanic reflex due to change in apparent resistance, or change in electro-motive force, or to both?

(2) Is the magnitude of the change dependent in any way on the actual resistance of the subject?

Physiological problems.

(1) What physiological changes can cause:
  (*a*) change in resistance;
  (*b*) change in electro-motive force?
(2) What physiological changes occur
  (*a*) during sleep;
  (*b*) during menstruation;
  (*c*) after physical and mental exercise;
  (*d*) after taking food
which could account for a change in resistance or electro-motive force?

(3) What part do the sweat glands, internal secretions, capillary action, thickness of the skin, play in the psycho-galvanic reflex?

From the point of view of psychology, there is overwhelming evidence that changes in affective tone are followed by galvanic deflexions indicating a rise in conductivity. We cannot say that every rise in conductivity is the result of an emotional change, since we have seen that other causes can have the same effect; yet we are safe in assuming that an affective tone above a certain level has a psycho-galvanic reflex corresponding to it, provided that physiological factors are not unduly disturbing the operation of the phenomenon. This does not mean that any given stimulus in any given subject will have the same effect, nor that the same stimulus will have the same effect with the same subject at different times; it only means that a given emotional state, however aroused, will have a corresponding galvanic deflexion. Further than this we cannot go with our present knowledge of the psycho-galvanic reflex.

In our opinion therefore the psycho-galvanic reflex is not a reliable means for measuring differences between individuals. The factors of error are so many and our present knowledge of the field of their operation so limited that it would be impossible to assign differences in the psycho-galvanic reflex to their specific cause with any degree of certainty. This does not, however, preclude the use of the psycho-galvanic reflex as a means of group differentiation. When using it in this way we are able to bring statistical methods to our aid by which we can determine the operation of chance factors. If the difference between two groups as measured by the psycho-galvanic reflex, is greater than any difference which could arise by mere chance, then we can assume that the difference is significant. Such a conclusion would be strengthened as a psychological measure if it were found to correlate positively with other tests indicating the same mental type.

Before we can go further in the use of the psycho-galvanic reflex as a psychological test we must await the result of physical and physiological research on the phenomenon.

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AN EXPERIMENTAL INVESTIGATION OF THE PSYCHOLOGY OF MORAL JUDGMENT

BY G. A. JOHNSTON.

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I. *The Purpose of this Investigation.*

THERE is an important group of problems on which ethical thinkers have been unable to reach any agreement, and, as their inability to attain any common conclusion seems very largely due to their unwillingness to supply themselves with those data without which the solution of the problems is impossible, it has seemed worth while to make the investigation the results of which are summarised in this paper. The purpose of the enquiry was to collect data which, it is hoped, will throw some light on four obscure and hotly-disputed questions. These questions may be very briefly stated. (1) How far are the moral judgments of plain men in agreement? (2) Are the plain man's moral judgments based on principles; and, if they are, what principles, if any, are explicitly or implicitly used by him? (3) Is the standard of moral judgment reason or feeling? (4) What is the place of deliberation and reflection in moral judgment? As thus summarily stated, some of these questions seem to overlap; but good cause will be shown in the sequel for keeping them distinct. The questions themselves have an ethical significance, but the task of collecting data on which to base answers to them belongs, it would seem, to experimental psychology.

II. *Description of the Method Employed.*

The data from which I hoped to be able to infer answers to these questions were obtained from replies to a *questionnaire* answered by 329 students, male and female, attending the Glasgow Training College. These were all Second Year students. In their first year they had had courses of instruction in logic and psychology. In their second year they all attended a course in ethics, but the answers to the *questionnaire* were all written at the very beginning of the session before the students had received any instruction in ethics. It was important for my purpose that the replies to the *questionnaire* should be given by people who had not been influenced in any way by instruction in ethical theory. In this way I obtained 329 papers with a record of the moral judgments of 329 people of average intelligence and education.

The students are divided normally, for tutorial purposes, into fourteen classes of approximately equal size, and I gave the *questionnaire* to each class separately. At the beginning of the hour I explained that I had five moral problems on which I wished to know their opinion. I said that I would read a statement of a certain action, and ask whether the man or woman in each case acted rightly or wrongly. Half-a-minute after I finished reading the statement of the problem they were to write a simple *Yes* or *No*. Then they were to have ten minutes in which to deliberate on the problem and state the reasons for their judgment. I made it clear that if, on reflection, they came to the conclusion that their original snap-judgment was mistaken, they were quite at liberty to change it. But they were to let the originally written *Yes* or *No* stand, in order to indicate that their view had been altered.

Exactly the same procedure was adopted in the case of all the problems. I read a statement of the problem, told my subjects when the first half-minute had elapsed, in order that they might jot down their monosyllabic answer; stopped them writing when the ten-minute period was at an end; and then read a statement of the next problem. (The statement of the problem was *read* in each case, to secure that the problem should be presented in exactly the same terms to every one of the tutorial classes.) The same time, *i.e.* $\frac{1}{2} + 10$ minutes, was allowed for each of the first three problems; for No. 4, $\frac{1}{2} + 5$ minutes was allowed; and for No. 5, which involves two questions, $\frac{1}{2} + \frac{1}{2} + 12$ minutes. The problems which I used were stated as follows:

(1) In Greek story, two brothers, Eteocles and Polynices, sons of the king of Thebes, agreed to reign in Thebes alternately, each for a year at a time. Eteocles, being the elder, was king the first year. At the end of the year he refused to transfer the sceptre to his brother Polynices. Polynices went away, secured assistance abroad, and marched against Thebes. Eteocles was besieged in Thebes, but eventually the two brothers met in single combat, and both were killed. The elders of the city decreed that Eteocles was to be buried with all honour; but Polynices was to remain unburied. Now, the Greeks believed not only that a man was deeply disgraced if his body did not obtain burial, but that until it was buried his soul would have

no rest in the land of the shades. The sister of the two brothers, Antigone, resolved to disobey the decree of the city, though the penalty for disobedience was a horrible death, and buried her brother. Did she do right?

(2) Sir Walter Scott published *Waverley* anonymously. Many people suspected that he had written it, and he was frequently asked directly if he were the author. For many years he steadily denied the authorship. Later, when he admitted it, he said that he had had no better reason for deliberately denying it than that such was his humour. Was he right to deny the authorship?

(3) A man, through no fault of his own, has been reduced to starvation. He is literally starving to death. He can get no work, and those from whom he begs will not give him either money or food. He therefore steals some bread. Is he right? (Do not trouble to ask whether such a problem could occur at present in our country. There are lands in which it might occur.)

(4) A man having bought a third class railway ticket travels in a first class compartment, though there is plenty of room in the third class carriages. Is he right?

(5) A doctor is attending a patient who is very dangerously ill. The doctor knows that the patient is of a very nervous disposition, and fears that the shock of knowing how critical his condition is will rob him of all chance of recovery. But the patient is anxious to know how serious his condition is, and persists in asking the doctor. The doctor tells him that his illness is not very serious, and that recovery is certain. Is the doctor right to tell this falsehood? Consider two cases—(*a*) the patient recovers, (*b*) the patient dies. Does this make any difference to the rightness or wrongness of the doctor's statement?

(6) (In the case of six of the classes the following was substituted for No. 5.) When Capt. Scott and his polar party were returning from the Pole their progress was retarded by the weakness of Capt. Oates. Capt. Oates thought that if the rest of the party were free of the necessity of slackening their progress on his account they might possibly reach the depot. He therefore walked out into the snow one night and lost himself. His companions knew he was going to meet his death, and did not stop him. (*a*) Was Capt. Oates right? (*b*) Were his companions right not to stop him?

Having mentioned the group of ethical problems on which I wished to throw some light, and the way in which the data were obtained from which I hoped to be able to derive answers to them, I now proceed to take the problems one by one, stating the implications of each, tabulating the results of the *questionnaire*, and drawing the deductions they seem to warrant.

III. *Uniformity and Diversity in Moral Judgment.*

The first problem on which our investigation seeks to throw some light is the question whether the moral judgments that are actually passed by men and women are really as divergent as they sometimes seem to be. Many thinkers who have dreamed of a really scientific system of ethics have been deterred from attempting to construct it by the apparently hopeless heterogeneity of the moral convictions of mankind. This is especially true of the interesting group of philosophers of the seventeenth and eighteenth centuries who hoped to see the foundation of a mathematical system of ethics. Descartes, Spinoza, Leibniz, Cumberland, Locke, and Berkeley all expected that it would be possible to demonstrate ethics mathematically; but practically all of them were forced to abandon the hope, because they believed that the lack of agreement among men on the fundamental principles of morals interposed a fatal barrier to the realisation of such a mathematical system[1]. For mathematics requires absolute unanimity and perfect agreement; and this, they came to believe, will never be possible in morals.

It seems worth while, then, to ask, How real is this apparent diversity? The *questionnaire* supplies some answer to that question. The *questionnaire* was purposely restricted, in the first place, to a homogeneous circle. These Training College students form a much less heterogeneous body than a class in a University would be. They vary very little in age, in social standing, in upbringing, and in education. In age, they are, with one or two exceptions, all between 20 and 23; they all belong to very much the same stratum of society; they have nearly all been educated on the same lines in Board Schools and Junior Training Centres under the Scotch Education Department; they had all in their first year at the Training College taken the same courses of instruction; and an overwhelming majority of them had come from Glasgow and the West of Scotland. It will be seen, then, that this body of students is relatively homogeneous.

The accompanying table shows the results of the investigation. This gives the percentage of students in each class who said that each action was right, and the percentage who said that it was wrong[2]. (Class K 2 consists of men; all the rest are women.)

It will be observed that, as might have been expected, the agreement of the moral judgments is much greater with regard to some problems than others. If we look at the percentage totals, we see that 100 per cent. of the students judge that the man who travels with a third class ticket in a first class compartment with no good reason does wrong. On this question there is absolute unanimity. Again, 91·5 per cent. judge that Antigone was right in what she did—a very considerable degree of agreement. On none of the other problems is there such a high per-

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[1] See Descartes, *Meditations, Reply to the Second Objection*; Spinoza, *Principia Philosophiae Cartesianae* and *Ethica ordine geometrico demonstrata*; Leibniz, *Nouveaux Essais*, III. xi. 17 and IV. xii. 8; Locke, *Essay*, III. xi. 16, IV. iii. 18, IV. iii. 20, IV. xii. 14; Berkeley, *Commonplace Book*, *Works*, I. 46, I. 37, I. 39, I. 55; and cf. Glanvill, *Scepsis Scientifica*, p. 179.

[2] These 'rights' and 'wrongs' were calculated from the *deliberate* answers to the problems. How the snap-judgments were utilised is explained below.

TABLE I.

Percentage of 'Rights' and 'Wrongs' in each class.

Problem		Classes														All students
		K 1	K 2	J 1	J 2	J 3	H 1	H 2	H 3	G 1	G 2	G 3	I 1	I 2	I 3	
1.	R.	90·0	85·7	86·9	81·8	91·3	95·0	100·0	95·2	91·3	90·9	96·3	81·0	100·0	95·7	91·5
	W.	10·0	14·3	13·1	18 2	8·7	5·0	0 0	4·8	8·7	9·1	3·7	19 0	0·0	4·3	8·5
2.	R.	16·7	32·1	21·7	4·6	8·7	5·0	8·7	9·5	34 8	31 8	25·8	28 6	21·7	26·1	20·6
	W.	83·3	67·9	78·3	95·4	91·3	95·0	91·3	90·5	65·2	68·2	74·2	71·4	78·3	73·9	79 4
3.	R.	46·7	67·9	30·4	31 8	39·1	45·0	56·5	42·9	56·5	31·8	55·5	33 3	34·8	39·1	44·4
	W.	53 3	32·1	69·6	68·2	60·9	55·0	43·5	57·1	43·5	68·2	44·5	66·6	65·2	60·9	55·6
4.	R.	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0 0	0 0	0·0	0 0	0 0	0 0	0·0	0·0
	W.	100·0	100·0	100·0	100 0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100 0	100·0
5 (*a*).	R.	86·7	89·3	69·6	81·8	95·7	85·0	91·3	85·7	100·0	72·7	92·6	81·0	87·0	95·7	—
	W.	13·3	10·7	30 4	18·2	4·3	15·0	8·7	14·3	0·0	7·3	7·4	19·0	13 0	4·3	—
5 (*b*).	R.	76·7	71·4	56·5	77·3	91·3	75·0	87·0	81·0	91·3	40·9	44·5	66 6	52·2	47·8	—
	W.	23·3	28·6	43·5	22·7	8·7	25·0	13·0	19·0	8·7	59·1	55·5	33·3	47·8	52·2	—

N.B. The numbers in the 5 (*a*) and 5 (*b*) rows in the columns of classes G 1, G 2, G 3, I 1, I 2, I 3 refer to problems 6 (*a*) and 6 (*b*) which in these classes were substituted for problems 5 (*a*) and 5 (*b*).

centage of agreement; and in the case of two problems the diversity is as great as it well can be: 44·4 per cent. of the students say that the starving man is right to steal, and 55·6 per cent. say that he is wrong; 57 per cent. say that Oates's companions were right to allow him to go and commit suicide, and 43 per cent. say that they were wrong.

On the whole, the diversity in the judgments of this homogeneous body is striking. It is startling to find that this large body of students is almost equally divided on the question what is the right thing to do in circumstances in which they might conceivably be placed. But, it may be objected, is the divergence of moral judgment so great as these figures would seem to indicate? Are statistics not misleading here? After all, it may be objected, your propositions, as Berkeley said of Locke's examples of a mathematical science of ethics, "are but trifling." If the problems had been real problems in ordinary life, then, as the railway problem suggests, the agreement of the 329 judgments would have been absolute.

I do not agree that the problems as a whole can fairly be stigmatised as artificial, remote, and casuistical. Problem 1 (the Antigone question) is the most remote of all from ordinary life, and yet, if the staging be overlooked for the moment, it is in essence a question with which not a few people in the twentieth century find themselves faced. And, it may be remembered that, in spite of the apparent remoteness and artificiality of the problem, remarkably little divergence of opinion was manifested by the students in their judgments upon it. They showed, on the whole, that they were quite capable of putting themselves in Antigone's place, and of looking at the matter from her point of view. None of the other questions was so far removed from ordinary present-day experience as this one.

We may now proceed to draw attention to one or two significant aspects of the results of our investigation. The table (Table I) gives not only the percentages for the 329 students, but also the percentages for each tutorial class. It seemed worth while to indicate the separate figures in each case, because each tutorial class is a College unit whose members remain constant throughout the whole College course, and thus each class tends to develop some sort of individuality of its own. The extent to which this individuality expresses itself may be gauged by the variation between the percentages of 'rights' and 'wrongs' in different classes. It should be mentioned that classes with the same nominal letter, *e.g.* G 1, G 2, G 3 meet together for some purposes in the College, *i.e.* G 1 comes much more closely into contact with G 2 than with (say) J 2.

In some cases the classes differ so greatly (*e.g.* while 68 per cent. of K 2 say that the thief is right, only 30 per cent. of J 1 agree in this judgment; and 35 per cent. of G 1 hold that Scott is right, while only 5 per cent. of J 2 agree), that it might be suspected that the problem was not stated in precisely the same way in the different classes. But this suspicion is quite gratuitous. The statement in each case was read, and precisely the same times kept in each class. The difference must be attributed to the group spirit, which the class has developed in the year during which it had formed a corporate unit in the College, and which makes each class look at the moral problems in a slightly different way from any other.

The results of the experiment seem to show clearly that certain classes have unconsciously adopted an attitude to life which expresses itself in all their answers to the problems. It will be remembered that the problems are all alike in this, that they ask whether an action that is formally wrong, wrong, that is, according to some rule or law, is really wrong. Now the more conventional and less independent a class, the more likely is it to hold that an action which is formally wrong is really wrong. On the other hand, the class which is independent and unconventional will be inclined to maintain that even though an action is inconsistent with formal laws, whether moral or legal, it may yet really be right. Now, on glancing at the percentages in Table I, we observe that K 2 varies very considerably from the student-body as a whole, and, in particular, contains a very much larger proportion of 'rights' than the other classes. It is much more ready than the others to judge that an action which, formally considered, is wrong is really right. We may therefore infer that K 2 as a class is distinctly unconventional and independent in its judgments. On the other hand, J 1 and J 2 (whose percentages are strikingly similar) diverge considerably from the student-body as a whole in the opposite direction. They are much less prepared to assert that an action which conflicts with rule or formula is right. These classes are markedly conventional and timid in their moral judgments. K 1, it may be pointed out, is clearly a very well-balanced class, diverging very little either one way or the other from the general view of the students as a whole. In the other classes we see various degrees of divergence from the normal, which, showing that some classes are persistently on one side of the general view, while others are persistently on the other side, indicate the formation by each class of a definite moral attitude, and illustrate the influence of members of the same community upon the development of one another's moral judgments, and the growth of a genuine community-standard of judgment.

From the standpoint of group-psychology another interesting result follows from our data. On the whole, the judgment of any one class agrees more closely with the judgment of the group of classes to which it belongs than with the judgment of all the students taken together. For example, the percentages of J 1, J 2, J 3 each approximate much more closely to the percentage of J 1 + J 2 + J 3 than they do to the percentage of all the students taken together. This is also true, though it is not so strikingly apparent, of the relation of the other classes to their groups. The fact that J 1, J 2 and J 3 are associated more closely with one another in the work of the college than they are with other classes seems to have had an influence on the formation of their moral attitude.

IV. *Principles implied in Moral Judgment.*

The second main question to which we seek to give some answer is, "Are the moral judgments of ordinary people based on principles; and if they are, what principles are implied?" The students were asked to give reasons for their judgments, but, in doing this, very few of them made reference to anything that could be called a moral principle. A good many seemed to be at the level of proverbial morality, and in support of their judgments they frequently quoted proverbs. *E.g.* in the 'doctor' problem, in the case where the patient recovers, "the end justifies the means" and "all's well that ends well" were often cited as evidence that the doctor did right to tell the lie.

Several students stated quite explicitly their conviction that there are no universal moral principles. In dealing, for instance, with Problem 1, some said it was an unreal problem to us, because its solution depends on any standard *we* like to apply, whereas it was decided by Antigone according to *her* moral standard. But, say these students, there is no real similarity between our moral principles and those of Antigone's day. Moral standards, they believe, have reference only to the particular conditions of particular times. There is, on the whole, in the minds of the students, very little recognition of the possibility of the existence of universal and ultimate moral principles.

There seemed, however, to be fairly general agreement among those who mentioned any principle at all that an action is right if its 'motive' is right. In these answers no other word is used with nearly such frequency as 'motive.' Many students agree in saying that the action is right because the motive is good, though they do not state either (*a*) what they mean by motive, or (*b*) what they mean by a 'good' motive.

(*a*) The one or two who do try to explain what they mean by motive are very confused in their analysis. It is clear, however, that for some the motive with which an action is done is equivalent to the end for the sake of which it is done, while others take motive to mean the feelings which inspire, encourage, and accompany the action. This confusion, of course, is one into which ethical writers not infrequently fall.

(*b*) Few students take the trouble to express what they mean by a 'good' motive, but those who do connect the goodness of motive, almost without exception, with 'unselfishness' and the badness of motive with 'selfishness.' In the judgment of many of these students the moral law might be summed up thus, "What is selfish is wrong: what is unselfish is right."

But, while motive is the most popular term with the students, it is not the only one to which they appeal. Some of them refer to the 'intention' with which the action is done, and hold that if the intention is good the action is right.

The words 'end,' 'result' and 'consequence' are also used by the students, but without any attempt to distinguish them. When I divided Problem 5 into two parts, I had this matter in view; for I hoped that the students would be forced to distinguish the 'end' from the 'result' and the 'purposed consequence' from the 'actual consequence.' This double problem gave the students considerable difficulty, and a glance at Table I will show that in every class the death of the patient in the latter case was held by some students to change the doctor's action from right to wrong. Over the whole body of students who did this problem 85·8 per cent. maintain that the doctor is right to tell the lie in the case where the patient recovers, but the percentage drops to 76·8 per cent. in the case where the patient dies; *i.e.* 9 per cent. of the students believe that the non-realisation of the expected consequence of the action does make a difference in its moral character. Yet while the figures prove that this is so, very few of the students seem to have any awareness of the distinction between the end aimed at in the action and the actual consequence that results from it.

The only other principles referred to in these papers are 'conscience,' 'instinct,' 'love' and 'vocation.'

Some students by implication, and that in two ways, indicated their belief in the existence of permanent moral principles. (1) Some drew attention to the conflict between moral principles. They point out, for instance, that Antigone was obeying the law of nature rather than the arbitrary law of man. "Mortal law has no right to condemn a man forever." Again, in the case of Oates, there was a conflict between the law of his conscience and 'the statute law.' Such conflicts bear witness to the existence of moral principles. (2) Other students assert the existence of definite classes of actions which are exceptions to the general rule which forbids them. It was freely admitted, for instance, that lying is wrong, but several people asserted that an exception may be made in favour of 'professional lies' ("such a lie is part of the doctor's medicine-chest") and 'social lies' (*e.g.* "Not at home," "So glad to see you"). These are classes of exceptions to the general rule, which still remains valid. Now, of course, though the students do not explicitly make this point, the very existence of these exceptions points to the necessary existence of a principle of which they are exceptions. *Exceptio probat regulam.*

On the whole it may be said that the students make only a limited use of anything that might be called a moral principle.

V. *Is the Standard of Moral Judgment Reason or Feeling?*

We must next ask, What light do these answers throw on the important question whether the standard of moral judgment is reason or feeling? Do such principles as have been enumerated imply one or other standard? On the whole, while the students never use the terms 'basis of moral judgment' or 'standard of moral judgment,' the principles which they mention suggest that the standard is reason rather than feeling.

Only a few students imply that feeling is the standard of judgment, and those who do this refer to the matter chiefly in connection with Problem 1. One girl, for instance, says, "This is a question which can't be decided by logical reasoning: we *feel* the fitness of it, even if we can't prove it logically." And probably most of those who justify the actions on the ground that they are the expressions of a natural instinct, or show love and affection, would agree that the ultimate moral standard is feeling. But these are exceptions.

The great majority imply that reason is the standard of moral judgment. They hold that moral judgment is *judgment*, that it requires reflection, and that it is based on rational grounds. Sometimes it is even assumed that if one has a good enough reason for doing wrong, then wrong may become right. "A lie is not a lie, if you have a good enough reason for telling it." Many insist that an action is right only if it is deliberate, and embodies a reasoned purpose. It may safely be said that 85 per cent. of the students believe that reason is the basis of moral judgment.

Now, while there is very general agreement that reason and not feeling is the basis of moral judgment, a good many students believe that there are reasons and reasons; that some reasons are better than others; and that, though two actions may both be right, there may be better reasons for the one than for the other, and thus one action may be more right than another. Hence there may be degrees of rightness and wrongness. Several, for example, of those who say that the doctor is wrong in both cases in telling the lie affirm that in the second case his action is 'more wrong' or 'a worse sin.' And if the quantitative results of this enquiry mean anything, they would seem to indicate that Scott's action was more wrong than the thief's, because whereas 79·9 per cent. judge Scott wrong, only 55·6 per cent. pass a similar judgment on the thief; and that Oates's action was more right than his friends', since 88·7 per cent. declare that Oates was right, and only 56·9 per cent. that his friends were right. The investigation therefore not only suggests that reason is the basis of moral judgment, but would seem to point to the existence of differences of degree in rightness and wrongness.

VI. *The Influence of Reflection on Moral Judgment.*

Our next problem consists in discovering in quantitative terms the influence of reflection and deliberation on moral judgment. As we have already seen, a large proportion of the students hold that moral judgment is based upon reason. Now, if moral judgment were merely a matter of feeling, it would be in accordance with expectation that the snap-judgments would, however long a period were given for decision, remain unchanged, since the verdict of feeling is always immediate. On the other hand, if moral judgment is based on reason, a period of reflection will naturally sometimes lead to the alteration of the snap-judgment. The *questionnaire* was so arranged, it will be remembered, as to make it possible to calculate the number of changes in the judgments due to deliberation and reflection. In Table II the results of this enumeration are shown. I have stated the percentage of changes (*a*) from 'right' to 'wrong,' and (*b*) from 'wrong' to 'right' for each class and also for the body of students as a whole; and also the percentage of all changes to judgments made.

TABLE II.

Changes in Judgment (*percentages*).

	Classes														
	J 1	J 2	J 3	H 1	H 2	H 3	K 1	K 2	G 1	G 2	G 3	I 1	I 2	I 3	Total
R. to W.	2·9	4·5	2·9	5·8	4·3	2·4	2·2	5·7	5·8	3·7	2·5	6·3	3·6	7·2	4·2
W. to R.	14·5	12·8	18·7	16·6	12·3	15·1	3·8	7·5	15·9	17·4	10·5	12·5	12·3	5·8	11·7
Total	17·3	17·3	21·5	22·4	16·6	17·5	6·0	13·2	21·7	21·1	13·0	18·8	15·9	13·0	15·9

NOTE. R. to W. = Right to Wrong. W. to R. = Wrong to Right.

Attention may be drawn to one or two points in the table (Table II). (1) No less than 15·9 per cent. of the snap-judgments were altered on reflection. That almost one-sixth of the original judgments were changed after deliberation supports the conclusions of § V that moral judgment is not based upon feeling. Moral judgment cannot be merely a matter of instinct, immediate perception, or intuition, for in that case such a large proportion of alterations on reflection would be inconceivable. (2) It is interesting to note that the changes from 'wrong' to 'right' outnumber those from 'right' to 'wrong' by nearly 3 to 1. If the general character of the problems be borne in mind, the significance of this becomes clear. In every case the action to be judged appears at first sight to break some more or less conventional moral rule. Hence the figures show that, while the snap-judgments imply conventional standards, reflection has shown these conventional standards to be inadequate. Although the action, judged by convention, appears wrong, yet after deliberation it is found to be right. Here also the results of the investigation emphasise the importance of the element of reason in moral judgment.

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THE PRESENT STATUS OF THE CONCEPTS OF NERVOUS AND MENTAL ENERGY

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II. *Energy-manifestations in peripheral nerves* (pp. 266–269).
III. *Energetics of the central nervous system* (pp. 269–273).
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Literature (278–279).

I. *Definition.*

It is necessary to start with a clear conception of physical energy. When a body or a system can do work against a force it is said to possess energy. A measure of the energy is obtained by regarding the decrease in energy as equal to the work done. "It is important to notice that although we can conceive the idea of a given substance or a system possessing energy, we are unable to give this a numerical value. What we can do is to ascribe a numerical value to the change in energy involved in a given process" (Lewis (1)).

The quantitative expression for energy is given by capacity × intensity, or $F=\frac{ML^2}{T^2}$ when F is the force which moves the mass M through the distance L in time T (and when $F = MV$, when V is the velocity which F imparts to M).

II. *Energy-manifestations in Peripheral Nerves.*

Experimental results in this field were until recently of a negative kind. It was natural to seek in nerve the evidences of metabolic changes —oxygen consumption and carbon dioxide output—such as are found in other tissues. Attempts to demonstrate consumption of oxygen measurable even by the most delicate methods, have failed. Indirectly, however, various workers of the Verworn School claim to have demonstrated the necessity for a certain small quantity of oxygen for the functioning of nerve—or at least, for prolonged functioning. Thus H. v. Baeyer (2), by enclosing a nerve in an atmosphere of pure nitrogen, succeeded in producing loss of function with complete and rapid recovery on restoration of oxygen. Fröhlich (2) showed that lack of oxygen prolonged the refractory period. Thorner (2) found that the irritability of repeatedly excited nerve sank much more quickly in the absence of oxygen and further claimed to obtain even in atmospheric air the indications of fatigue—extension of the excitation curve, with consequent apparent summation, and reduced irritability. These experiments, however, go no further than to show that oxygen may be somehow necessary for the prolonged functioning of nerve. It would be necessary to show that certain of the effects described were not due simply to narcotisation. Nor do the results demonstrate what kind of energy is involved. If a true oxidation is concerned, heat and CO_2 production are implied. Tashiro (3) has claimed to furnish evidence that CO_2 is given off during excitation. But his evidence has been criticised by Bayliss (4), who points out, among other objections, that such small quantities of CO_2 as those demonstrated by Tashiro could be the result of the respiration of the connective-tissue cells in the nerve-trunk.

As to the heat-production which would be associated with an oxidation process, A. V. Hill (5) with a method which measures a temperature change of one six-millionth of a degree, could detect no effect after 25 seconds continuous stimulation. This result means that a single propagated disturbance does not result in the production of more than $1^\circ \times 10^{-8}\,^\circ$ C., *i.e.* one hundred-millionth of a degree. Hill concludes, therefore, that it is impossible that an oxidation process can be involved in the propagation of the nerve-disturbance. Further evidence is provided by the temperature coefficient of the rate of conduction of the nerve impulse, which is 1·79 for 10° C. (Keith Lucas (6)). Bayliss (4) points out that so low a coefficient alone makes it extremely improbable that a chemical reaction is involved.

Heat and chemical energy being excluded from the explanation of the propagated impulse, it is necessary to turn to the other known forms.

The earlier notion, that the passage of the impulse might be a case of simple electrical conduction, was negatived by v. Helmholtz's (7) demonstration of the rate of conduction as not more than 29 metres per sec. (This has since been corrected to 33 metres per sec.) In some invertebrates, speeds as low as 1 mm. per second have been observed (8). The discovery, however, of the electrical change in nerve accompanying the passage of the impulse together with the demarcation-current and the electrotonic phenomena facilitated the investigation of the phenomena of conduction and led indirectly to theories which have avoided the above difficulties. Of the processes mentioned, none are peculiar to nerve-tissue. There has been some dispute as to whether this current of action in nerve and the actual nerve-impulse are identical phenomena. Keith Lucas pointed out that while none of the objections are valid in themselves, caution is still necessary in accepting their identity. It is also necessary to remember that the impulse studied in the isolated nerve may not be the same phenomenon as that occurring in the intact animal. Adrian (9) and Miss Cooper working on reflex contraction, and Bumke (9) working on inhibition, have recently brought evidence which tends to confirm their identity.

The electrical phenomenon in nerve was more or less closely imitated by Hermann (10) and Boruttau (11) by means of their 'core-model'; but their explanation presented certain difficulties, *e.g.* it did not satisfactorily explain the electric variation accompanying the transmission of the impulse. These difficulties have been avoided by the physico-chemical theories, of which Macdonald's (12) is an example. Macdonald regards the nerve-fibre as a 'concentration-cell,' the salt concentrated within the fibre being assumed to exceed that in the tissues bathing it, and the fibre to be surrounded by a semi-permeable membrane, at the surface of which a Helmholtz 'double-layer' is formed (*i.e.* cations on one side of the membrane, anions on the other, the membrane being impermeable to each in one direction). In this theory, the ions have to be assumed to be partly absorbed on the surface of colloids, to prevent their exerting osmotic pressure. Diffusion of one kind of ions, no longer restrained, explains the current of injury between the cut end of a nerve and its injured portion. The negative variation of the current of injury which occurs on stimulation of the nerve would be explained by the sudden permeability of the membrane to the other kind of ion, the electrical potential difference between opposite sides of the membrane disappearing, from free mixing of the ions.

The general trend of this hypothesis is supported by the Nernst (13) theory of excitation, which was deduced from the failure of currents of short duration to excite nerve. Nernst assumed that a certain minimal concentration of ions at an interface which necessitated a certain time for the concentration to be obtained, was required for stimulation to occur. He accordingly invented a formula which, as adapted by Keith Lucas for nerve (where numerous membranes must be invoked at short intervals from each other in successive lengths of the nerve), was found to fit some of the experimental data.

Lillie (14) has recently performed some interesting experiments, in which he showed that when an iron wire is immersed in an electrolytic solution (HNO_3) a film of electrochemically alterable material is formed on the surface of the metal. When the wire is activated by touching it with *e.g.* a piece of zinc at one end, a wave of change in the surface-film passes down the wire, and is visible as a progressive darkening of the hitherto bright surface. This change is reversible, and Lillie believes the phenomenon to be due to local bioelectric currents occurring successively in the length of the wire.

To conclude this brief résumé of the present conceptions of the nature of the phenomenon occurring in peripheral nerve: it is apparent that they are based on the physico-chemical properties of matter, especially the colloidal constitution of cells, with surface adsorption of ions, the presence of semi-permeable membranes in the nerve-fibre, and the limited diffusion of salts within the latter. The position of the physico-chemical hypothesis is well put by Adrian (9) when he says, "If we use the word 'energy' in a purely physical sense, the conception of a separate kind of nervous energy is unnecessary."

III. *Energetics of the Central Nervous System.*

The problem here is in some ways more easily attacked, but in others it presents more difficulty; for in the central nervous system, there are not only nerve-fibres, but also nerve-cells and the innumerable interconnections of the nervous elements.

That more tangible energy-changes occur in the central nervous system than in the peripheral nerves, is suggested by various anatomical and physiological conditions. The blood-vessels of the brain and cord are very numerous, and they are more plentiful in the cellular grey matter than in the white conducting tissue. That the absence of oxygen rapidly leads to loss of function in the brain and cord, and in the higher centres more rapidly than in the lower, is well-known. The blood coming from the brain is poorer in oxygen than that going to it. The experiences of Haldane in experimentally-produced anoxaemia are also illustrations of this.

The matter is not, however, so simple when we consider functional activity greater than the basal activity of rest. It is true that an increase in the blood-supply of the brain (Mosso (15)) and an increase in brain-temperature in consequence have been demonstrated during mental activity, and that conversely in sleep the blood-supply of the brain is diminished (Howell (16)) unless the sleep be of a disturbed kind (McWilliam (17)). It has also been shown that an increase in general arterial pressure and pulse-rate occurs during mental activity, under carefully controlled conditions (Gillespie (18)). It is, therefore, all the more surprising to find that metabolic experiments during mental work fail to show any increase over the basal value of rest (Benedict (19)). Becker and Olsen (20) claim to have shown that mental activity causes an increase of metabolism, but no confirmation of their work has been recorded.

As a further example of the comparative lack of correlation between general metabolic changes and mental activity, there may be cited the experiences of Benedict (21) and his co-workers, who found that their subject on the 31st day of his fast was as capable of mental work (as judged by performance tests) as he had ever been. This merges on the field of fatigue, and the lack of satisfactory tests of mental fatigue has to be remembered in evaluating these experiments.

The problem of the nature of mental activity has been attacked also by submitting the whole organism to performance tests. Adrian warns us that while there are analogies (if for example mental energy be regarded as the product of the 'force' into the 'resistance' overcome, although both are imponderables) there is no reason to suppose that this mental 'energy' will follow any of the generalisations which have been observed for the energy change in material systems.

It is accordingly interesting that many investigations of part-performances of the nervous system have been dominated by thermodynamical analogies. Thus Ebbinghaus (22): "The same inhibitory relation which exists between purely mental states and processes, occurs also between them and bodily movements. In medium degrees of attention both may quite well occur simultaneously. As soon, however, as on the one side the energy of activity is considerably increased, so soon will the other side suffer a decrease." This theoretical statement seems to be supported by Lehmann (23), who, using a finger ergograph, demonstrated a mutual interference of muscular and mental work. He went further and stated that brain-work of a constant magnitude caused in the same subject a constant reduction of a contemporary muscular innervation and that the relative diminution in the muscular work was a relative measure of the amount of brain-work. The methods which Lehmann used do not seem, on a perusal of the account given, to have been of the accuracy desirable for such deductions to be made from them: nor did they admittedly allow of observation of the converse effect of muscular on mental work. In a similar investigation with different methods (unpublished data) in which the latter defect was remedied, Dawson and I found in most subjects a mutual reduction of mental and muscular output, but not at all of the constancy described by Lehmann. Lehmann's experiments were very similar to Loeb's (24) performed many years before. Loeb came to very similar conclusions to those of Lehmann and devised a formula to express it. $\frac{P-p}{P}$ = constant (of attention) when P = maximum pressure exerted on a dynamometer without concomitant mental work and p = maximum pressure with concomitant mental work. P is thus the expression of the attention when the whole available attention is given to the work.

These experiments of Loeb's were confirmed by Welch(25), but her methods are so open to contention that they are not of much confirmatory value. She omitted discordant results, and used only one subject and that subject herself, and she knew the results expected.

McDougall's(26) 'drainage of energy' hypothesis probably also implies in an indirect way, the first law of thermodynamics and the further hypothesis of the presence of a constant supply of energy in the nervous system which is available to all parts of it. However useful this hypothesis may be in psychological discussion, it does not conform, as Bayliss points out, to physiological facts, *e.g.* with regard to the phenomena of inhibition, which it was supposed to explain.

Other workers have found precisely the opposite conditions to those described by Loeb(24) and Lehmann(23). Mosso(27), Féré(28), Fechner(29), Patrizi(30), Corbei(31) and Dresslar(46) all describe 'dynamogenic' effects as the result of the simultaneous action of different muscular processes, or muscular and mental activities. They advanced various hypotheses varying from Mosso's "peculiar property of our organism" to "diffusion of excitement" and "increased muscular tensions."

The physiological concepts of optimum ionic concentration and increased permeability of membranes resulting in diffusion of excitations begin to make such results understandable. The fact that muscular and mental work may sometimes be done simultaneously without apparent diminution in the output of either, suggests that just as there are conceived to be physiological levels of function in the nervous system, it is possible to regard the energy of the central nervous system as being also stored at various levels, so that there is no necessary withdrawal of energy from one of these to another when two levels are simultaneously activated. The suggestion that there are different *kinds* of mental energy, "the one explosive and intermittent, and the other adaptive and continuous," has been formulated by C. S. Myers(32). He suggests that roughly analogous to two varieties of muscular activity—contraction, and the maintenance of tone (which physiologically costs less)—there are these two varieties of mental activity, the explosive intermittent variety, resulting from the need to make numerous judgments and decisions, and the other adaptive, continuous one arising from the need to maintain a certain mental set or attitude and sustained by interest. It might be possible to find a mental analogy in the form of strained attention as in listening for an expected auditory stimulus, to the third and most costly type of muscular activity, viz. static effort.

The problem of the nature of nervous energy has also been explored in an indirect way by the experimental neurologist, and it is of some interest to compare his observations with those from other sources, and to see how far clinical and experimental phenomena can be explained on a physico-chemical basis. Head(33) has shown how, after injury or transection of the spinal cord, all reflexes below the level of the lesion are at first abolished, but gradually return, in increasing complexity and with more diffuse excitability both as to their field of stimulation and their response. It seems that this would fit well with the hypothesis of the varying permeability of membranes at synapses, the increasing differences corresponding to recovery of permeability. The fact that narcotics, chloroform for example, which are known to hinder or even abolish conduction by their effects on membrane permeability, cause a return to an inexcitable condition, and that as the effect of the narcotics passes off the diffuse excitability returns, is in accord with the physico-chemical hypothesis. The action of strychnine, although as yet its ultimate nature is unknown, in facilitating the passage and diffusion of impulses, is also an illustration. Head calls the degrees of extent of diffusion of impulses in the spinal animal, degrees of 'vigilance.' He also applies this term to the more complex phenomena seen in the decerebrate animal, and regards the purposive reactions of this animal as examples of the highest degrees of vigilance. It is in this respect that 'vigilance' differs from mere excitability—in that it includes the complication of responses and not mere differences in stimulus-threshold. But these complicated 'purposive' functions can also be progressively abolished under the influence of narcotics. It does not therefore necessarily follow that the functioning of the higher organisations differs in kind from that of the lower, especially if the hypothesis of differences of synaptic resistance be admitted, and if it be remembered that complicated schemata of any kind, mechanical, physico-chemical and the like are more easily disordered than simple ones. Head prefers to regard the differences in grading and discrimination of response and in sensibility to toxins at the different levels as differences in the degrees of 'vigilance.' This is a behaviouristic expression for what he terms at other times 'neural potency,' 'vitality' and 'physiological efficiency.' This question of 'vigilance,' 'physiological efficiency,' 'organic regulation,' will be considered again.

When we pass from the motor and reflex phenomena of the central nervous system to the consideration of the physico-chemical explanations advanced for mental processes, the ground is yet more uncertain. The disturbances in mental activity produced by oxygen-deprivation and by narcosis, are too well known to need more than passing mention. It is, however, interesting to notice that just as the Nernst formula deduced from the consideration of electro-chemical systems was found to have a useful application to the phenomenon of peripheral nervous conduction, so Brailsford Robertson (34) has observed that the formula for autocatalytic reactions holds also for the improvement of memory with repetition. This lends support, however nebulous, to the hypothesis that the processes of memory are physico-chemical reactions of a complicated kind, and is in accord with the observations on physiological memory made in other ways—for example, Haldane's (35) "memory of the respiratory centre," and the dependence, which Head emphasises, of each step in a complicated reflex on the steps that have gone before.

So far then as the central nervous system is concerned, difficulties in the explanation of the phenomenon are met with not dissimilar to those in the peripheral nerves. But from the importance of a supply of oxygen, and from the effects of drugs and narcotics as well as from observations on the mechanisms of the integrated nervous system and from various analogies, it is apparent that physico-chemical processes of varying degrees of complexity form at present the most reasonable bases for our working hypotheses; and the processes of the central nervous system are, in many instances, accompanied by consciousness and are then called mental. The present limitations of physico-chemical explanations are of course obvious; they are tentative and not final; and their limitations will be further apparent in the discussion of the detailed implications of the conceptions of 'vigilance' and 'physiological efficiency.'

IV. *The use of the Energy-concept in Psychology.*

It seemed desirable to deal with the physiological and physico-chemical methods in the investigation of energy changes in the nervous system before passing to the psychological conceptions of nervous and mental energy, which approach the problem from an entirely different angle, but are yet apt to embody physical analogies. It is not proposed to discuss whether the concept of energy is necessary for psychology, but simply what forms the concept has assumed in the hands of certain writers.

Academic and pathological psychologists are alike accustomed to introduce an energy-hypothesis. For some, the matter is simple. Kempf (36) for example finds the origin of psychical energy in muscular tensions, although how a muscular tension (which is known to be concerned with the using-up of energy) can give rise to a fund of energy, in other words how a process which necessitates the consumption of energy (except in certain circumstances) can be at the same time energy-producing, is a mystery. If all that is meant to be implied is that muscle-tensions initiate psychical processes, the matter is different, and the problem of the nature of psychical energy has not been touched.

The assumption common to most psychological and psychopathological theories is that of a fund of energy. Whether this is a freely transferable fund, or whether it is more or less divided and its component quantities limited in their utilisation, is a question which is answered variously. Adolf Meyer (37) regards the matter as one of "initial general responsiveness, vigour of choice and sound habit," rather than of a set quantity of energy attached to a given instinct. McDougall (38) suggests that a combination of the two views, one of which postulates a common fund of energy for all the instincts, and the other a non-interchangeable quantity of energy attached to each, may be found most satisfactory. In any case, the organism in action is necessarily a unity, and all the utilisation of energy at a given moment is at the service of one act only, and the discharge occupies one final common path.

Freud avoided the difficulty of the partition of energy by reducing the fundamental strivings of the organism to one—the 'sexual,' in the psychoanalytic sense. There are held to be part-instincts within the sexual between which energy is interchangeable (40).

In the *Traumdeutung* Freud (39) uses a wealth of physical analogies and physically-derived hypotheses—such as facilitation and intensity of processes, and gradations in resistance—which he does not specially correlate. The hypothesis specifically of energy is closely associated with his topographical view of the higher sensory-motor arc. Thus there is assumed to be a stream of energy flowing from the perceptive to the motor side during waking life, and in the reverse direction during sleep. Freud also assumes that certain systems are invested with a certain amount of energy, which is discharged when the system is excited. The unconscious wish is supposed to have a large energy-investment. But the wish can be 'forced back' into the unconscious (an aspect of the topographical hypothesis in which a division is made into conscious, foreconscious and unconscious). The 'dream-work' seems to consist in a transference of intensity from one idea to another. Further, ideas are supposed to attract each other; which is an analogy from a different field.

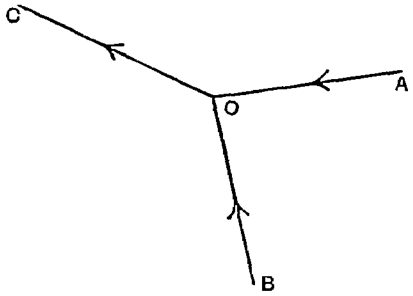
In his later work (41), Freud utilises Breuer's analogy of 'free' and 'bound' energy; but while in one place (p. 24) he attributes the former type to the conscious, in another (p. 33) it seems to belong to the unconscious, while the conscious contains only bound energy. It is the function, according to Freud, of the upper layers of the psychical apparatus to convert the free energy (including the instinctive energy from the unconscious and the energy from the environment) to the 'bound' condition. This is effected (on p. 24) by allowing free passage to excitations by diminution of all resistances; but on p. 27 what happens is that free energy is converted to the bound condition by means of a "contra-investment of energy." Apart from the unexplained duplication of hypotheses, Freud seems to confuse the physicist's sense of 'bound' (*i.e.* for the time being unavailable) energy with the literal sense of the local limitation of a disturbance. (Thus he speaks on p. 27 of "circumscribed limits" of irruption, and of an accumulation of 'contrary' energy in the 'neighbourhood' of the breaking-in).

A further inconsistency is implicit in his comparison of the life-instincts to the assimilatory process of Hering, and of the death-instincts to the dissimilatory (p. 47). For the assimilatory process is one of 'binding' in the physical sense, and the dissimilatory process is concerned with the freeing of energy: so that now both kinds of energy are attributed to the unconscious (since the instincts of life and death alike belong to the latter).

Janet (42) makes use of two principal hypotheses, psychological force and psychological tension; purely psychological conceptions analogous possibly to the capacity and intensity components of the physical equation. He analyses human tendencies and finds them to constitute a hierarchy, a conception reminiscent of Head's 'vigilance' in its various degrees. The more exalted the tendency, the greater its psychological tension. Tendencies have also a psychological force "by which I mean," says Janet, "that they can determine movements more or less powerful, rapid, and prolonged." He conceives this "force psychologique" as existing in two forms—(1) a latent or reserve form which (2) can be activitated when the tendency is aroused and commences to act.

Implicit in the concept of different mental levels, is a supposition such as has been suggested above, that the 'force' is stored at various levels. He says, "The old elementary tendencies have much force at their disposal, while the higher and more recently acquired tendencies have less..." (II. 292). This is a little difficult to understand in relation to his statement that an act at a high level of tension is very costly, *i.e.* requires a great deal of force (II. 79). Janet adds, however, a 'drainage' hypothesis. A tendency to movement, for example, can be drained by a tendency having a higher degree of tension. The principal reserves of energy "are localised chiefly in deep-seated elementary tendencies, destined to protect in case of need the life of the individual or the race." The well-known hypothesis of the restriction of the field of consciousness in hysteria, has a bearing on psycho-energetics if it is regarded as a diminution in the 'capacity' factor.

If a more objective point of view than some of those described above is assumed, and if the behaviouristic attitude be adopted while at the same time allowing for its limitations, it is possible to construct the following schema. The 'dynamic' conception of the organism in relation to its environment makes the conditions analogous to a triangle of forces.



If AO represents for diagrammatic purposes the organism conceived as a body moving in the direction AO, and BO an external stimulus, conceived in similar terms but in the direction BO, then OC can represent the resultant, depending on the respective masses and velocities of AO and BO. The schema is, however, not meant to represent anything so crude as the simple collision of the organism with something in its environment. Abstractions must be made and AO be considered as a 'stream of activity,' with direction. (The analogy with physical processes is closer than would appear at first sight, when it is remembered that one of the factors in the equation $F = MA$ is a velocity factor and that 'velocity' in the nomenclature of physics implies direction, in contradistinction to mere speed.) The direction of the resultant *OC* will depend on the relative directions and momenta represented by *AO* and *BO*. If it be asked how a sensory stimulus can have direction that will affect the organism's direction of motion, it is only necessary to instance the light tropisms (Loeb (43)). As for the organism's being represented as a stream of activity, not even the mechanists are likely to quarrel with this; but what the mechanists do not take account of is the stream of activity conceived as a stream with direction (Haldane's 'vis directrix'), a direction usually regarded as 'purposive.' As a result of this conception of the organism as a stream of activity with direction, it seems unnecessary to postulate a 'will.' For if direction be conceded to organism (stream) and environment (stimulus), an additional force to "move the points of the rails" and so influence the direction of *OC* (Myers (32)) is superfluous. The problem of choice also arises here. Now 'choice' is evident even at the lowest sensory level—at the periphery—in the specialisation of sense organs to receive different ranges of wavelength. Further, if the principle of the final common path (Sherrington (44)) be invoked and if it be remembered that the sensorimotor arc, even at the cortical level, is a continuum, it is evident that the result of one of several simultaneous sensations will take command, and direct the organism's motor activity. This tentative scheme is advanced to illustrate the possibility of working with comparatively few factors, which are implied in some of the recent and more objective conceptions of the organism.

From the psychobiological aspect of the organism as a whole, it might be a suggestive analogy to regard the psychical factor as the intensity component, and the somatic factor as the capacity component, the product of which expresses the total energy of the organism. The use of analogies however—and it will be seen that nearly all of our conceptions of mental and even of non-mental nervous processes are on the basis of analogies—is one that is attended with some risk, unless it is remembered that the appropriateness of such analogies is dependent more on the nature of our thinking than on any inherent likeness in the things themselves. So obvious is the failure to reduce all mental phenomena to purely physico-chemical terms (although further elucidation in this direction is foreshadowed by the work outlined above) that Ostwald (45) was forced to postulate the existence of a special variety of energy, namely psychical energy, to account for them. It is important to remember that this difficulty of the limitation of physico-chemical explanation is not confined to problems of nervous and mental energy, but extends to the field of all biology (with which some would now consider 'mind' in the widest sense, as co-extensive). The extraordinarily delicate regulation of breathing, which has been so well demonstrated by Haldane (35), and the fine discrimination exhibited in the excretory activities of the kidney, are only two of numerous similar cases. The "teleological ordering" of these activities is still a mystery, which is only now being freed of the obscurity cast upon it by the vitalistic hypothesis. Not only is there an apparent teleology, but the delicacy of discrimination exhibited in these phenomena far exceeds anything in the realm of physics and chemistry. These reactions, as Haldane says, are examples of the direct expression of the nature of the organism. To him it seems that the only just conclusion is that there is a biology whose manifestations not only differ in appearance, but are dependent on fundamentally different processes from those known to us in the realm of physical chemistry. "Try to conceive," says Haldane (46) "the development of the organism from the germ-cell in terms of the mechanistic philosophy." "Just as we can describe electrical energy," says Myers (32), "only in lower terms, *e.g.* inertia, mass and weight, so in using physico-chemical formulae to fit the phenomena of nerve and the central nervous system we may well be describing what is really a degradation of a special kind of energy unknown to us"—'neuro-psychic energy' if you like, but eventually a manifestation of the same thing as Haldane's 'vis directrix' or 'organic regulation' and Starling's (47) 'wisdom of the body.' It would be well in the meantime, therefore, to desist from using the term nervous energy, and to speak only of 'nervous activity.'

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THE EFFECT OF BINAURAL PHASE DIFFERENCES ON THE LOCALISATION OF TONES AT VARIOUS FREQUENCIES[1]

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(With Plates I—IV.)

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[1] A summary of this paper was given at a meeting of the British Association (Psychology Sub-section), Liverpool, Sept. 1922

I. INTRODUCTION.

(a) *The localisation of sources of sound.*

THE problem of the localisation of sources of sound has given rise to much experiment, and, during the late war, when the chief, and often the only, evidence of the position of the enemy's aircraft, submarines, tunnelling parties, and artillery was the sound produced, the problem was attacked with renewed vigour. Almost all the known properties of sound waves were used as the basis for some method of localisation. For example, the time sound waves take to travel formed the basis of most of the methods of localising hostile artillery; the exact times at which the waves, produced by the firing of the gun, arrived at three or more points at known positions with regard to one another were registered, when the position of the battery was easily deduced. The property of reflection was the foundation of one of the ways of detecting the presence and position of aeroplanes. The sound waves were brought to a focus by means of a parabolic mirror, and the observer, who was fitted with a device for listening to the sound at the focus, moved the mirror about till the sound heard was a maximum. The axis of the mirror then pointed in the direction from which the sound waves came. Similarly the position of a submarine was found, among other methods, by taking advantage of the property of refraction. Suitable lenses, built into the sides of the ships engaged in anti-submarine warfare, focussed the sound waves, which were conducted through the sea from the submarine's engine. The position of the focus was stated to give the direction of the submarine. These methods, which make use of the properties of reflection and refraction, ultimately depend on judgments of loudness, but, as the variation in loudness is continuous, it is difficult to decide the exact point of maximum intensity. Other methods were accordingly evolved which took advantage of the fact that man has two ears. If a source of sound is situated in front of an observer at a point in the sagittal plane the amplitudes of the vibrations of the sound waves at the two ears will be equal, and the sound waves at each ear will be 'in phase.' The normal individual will, under these conditions, judge that the sound comes from some place in the sagittal plane. If now the observer turns his head to the right, the amplitude of the vibrations will be greater at the left ear than at the right, since the right ear is further from the source of the sound and is more or less shaded by the head; also the phase of the sound wave at the left ear will be in advance of that at the right ear. As a result of these differences the observer will judge that the sound comes from the left. The effect can be increased by providing the observer with sound collecting horns, or microphones, separated by a distance greater than that between the ears; and, if the sound collectors are mounted on a suitably rotating stand, it is easy for the observer, without moving himself, to arrange, by rotating the sound collectors, for any given sound to be localised in the sagittal plane. The position of the rotating stand then gives at once the direction from which the sound actually comes.

(*b*) *Factors determining the judgment of direction.*

All these methods of localising a source of sound make use of the physical properties of sound waves and their physiological effects; but—I say this with every reservation, for much of the work done during the war on sound-localisation has not, to my knowledge, been published, and so is not available for reference—the researches carried out when perfecting them give us little or no information concerning the various factors which enable us to form our judgment of the direction from which a sound comes.

In locating a sound of very short duration, the time factor may be available; but it cannot be of any assistance, in its simplest and most direct form, when the sound is continuous, though it may conceivably be at the root of judgments based on differences of phase. Our powers of localising a continuous sound, and more particularly a musical tone, probably depend on a number of factors. The chief of these may be:

(1) the pitch of the note;
(2) its purity and binaural differences of purity;
(3) its absolute intensity and the relative intensities at the two ears;
(4) binaural differences of phase.

In addition, it has been suggested[1] that we may depend on:

(5) tactual sensations produced by the sound vibrations on the two auricles and tympanic membranes;
(6) labrynthine differences of stimulation; and
(7) kinaesthetic sensations in the canals due to reflex head movements.

Factors (5) (6) and (7) will not be considered in what follows. The experimental evidence in their favour appears to be very slight.

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[1] Cf. Myers, *Text-Book of Experimental Psychology*, Part I, pp. 273–4.

(c) *Necessity for the determination of the effect of each factor separately.*

If we are to obtain adequate knowledge of our powers of auditory localisation it is important that carefully controlled experiments should be carried out. First the effect of each factor, separately, should be investigated; then the relative importance of each factor should be considered; and lastly, the mass effect, which is probably very different from the algebraic sum of the individual effects, should be studied. In the present paper I propose to deal only with the question of the influence of binaural differences of phase.

II. PREVIOUS WORK ON THE PHASE EFFECT.

There has been a considerable amount of research into the effect of a phase difference between the sound waves at the two ears on the apparent direction of the source of the sound. Much of the work is, unfortunately, untrustworthy, so far at least as quantitative measurement is concerned, owing to lack of proper control. Two general methods have been employed, (*a*) dynamic, and (*b*) static.

(*a*) *The dynamic method.*

In this method continuously varying phase differences between the sound waves at the ears have been produced by leading to each ear separately sounds slightly out of tune with one another. A single fused sound is then heard, which appears to travel round and through the head as the relative phases vary. This method gives good qualitative results, but is not one which lends itself to accurate quantitative measurement, owing to the difficulty of stating the exact time at which the sound has a given lateral localisation; Stewart (16, 17), however, quotes the results of experiments made by this method (with forks of frequency 128, 256 and 512 d.v. per sec.) as confirming his other observations.

(*b*) *The static method.*

Practically all quantitative observations have been made by some form of static method, in which a certain phase difference between the sound waves at the two ears is produced, and the corresponding localisation of the sound is noted. The most common way of obtaining the phase difference has been to lead the sound from a single source, usually a tuning fork, to the ears by two separate paths: if the relative lengths of the paths are varied, the relative phases of the sound waves on arrival at the ears will vary also, and further the phase relationship can be readily calculated. This method has been employed by Myers and Wilson (11, 18), More (9), More and Fry (10), Simpson (14), and, partially, by Stewart (16, 17). The great objection to this method is that resonance may occur in the tubes which are used to lead the sounds to the ears, and since the tubes are in general of different lengths, the resonance effects, and all that they may involve, will not be the same on the two sides. That such resonance does occur, and that it may mask the phase effect, has been pointed out by Myers and Wilson (11, 18). Bowlker (3), also ascribed certain anomalous results he obtained in some preliminary experiments to the same cause. He used an organ pipe as his source of sound, and placed cylindrical aluminium tubes of various lengths over his ears, thus producing a difference between the phases of the sound waves at his ears. To minimise the resonance "large loose wads of cotton-wool were inserted at the outer ends of the tubes." But resonance effects being "still rather disturbing," he modified his method and, in his final experiments, used tubes of equal length which he held "quite closely" against his head. He obtained a difference of phase by turning his head round slowly, so varying the lengths of the paths the sound waves had to travel to the two ears. Bowlker's method is probably an excellent qualitative one, but it would seem to be rather too crude to yield accurate quantitative results. In a second general method the two ears have been stimulated separately by similar sources of sound which are under control so that, while the sounds remain in unison, their relative phases may be varied at pleasure. This method was first used by Rayleigh (13); but, having confirmed the results he had previously obtained with tuning forks and satisfied himself as to the importance of the phase effect in the localisation of sound at low frequencies, he experimented no further. His apparatus consisted of a magnet which was made to revolve, about an axis perpendicular to its length, at a speed of about 190 revs. per second. The magnet acted inductively on "two inductor-coils...the circuit of each being completed through a telephone. The planes of the coils were vertical, their centres being at the same level as the magnet. One was fixed and the other was so mounted that it could revolve about an axis coincident with that of the magnet....The angle between the planes represents, of course, the phase-difference of the periodic electromotive forces, subject it may be to an ambiguity of half a period, dependent on the way the connections are made." Stewart (16, 17) worked with a modified form of this apparatus. He had "a rotating toothed wheel with two telephone bipolar magnets placed radially close to the teeth and capable of being separated from each other by a variable number of degrees of rotation of the wheel. Currents of the same frequency and complexity were induced in the two bipolar receiver magnets, each of which was connected to a head receiver, and the phase difference of the currents could be controlled by the separation of the two bipolar receivers around the circumference of the rotating wheel." In this way differential resonance effects due to the tubes are eliminated, but they may have reappeared in the telephones.

(c) *Criticisms.*

Most of this work may be criticised on one or more of three grounds: (*a*) physical, (*b*) physiological, (*c*) psychological.

From a physical standpoint we have two factors besides differences of phase to consider, namely, (1) *The purity of the tone and binaural differences of timbre*; and (2) *Intensity effects.* With regard to (1) no experiment has, so far as I know, been performed to show that the phase effect has been obtained with demonstrably pure (objective) tones. Even with the dynamic method used by Rayleigh (12), we cannot be sure that the tones were pure. This doubt is increased with what I may call the Myers-Wilson type of apparatus, where differential resonance effects may creep in and produce binaural differences of impurity as the lengths of the tubes are varied. In Bowlker's experiments the tones were admittedly impure. With Stewart's 'phaser' no attempt seems to have been made to ascertain whether the notes were pure, or whether they were even of similar impurity. It is, therefore, possible that all the effects observed are due in some way to impurities in the notes themselves, and that the 'phase effect' is not due to phase difference directly. This question can be settled finally only if the phase effect is obtained with tones which are, beyond any question, pure. (2) *Intensity effects.* Anomalous results obtained by Myers and Wilson and by Bowlker, in his earlier experiments, were, as we have already seen, ascribed to differences of intensity. Whereas these differences of intensity were so marked as to mask, and even to reverse, the expected results in some instances, they may have been present at other times, and may have introduced variations sufficient to vitiate the observations entirely, at least so far as quantitiative measurements are concerned. It scarcely seems adequate to state generally that a sudden decrease in the intensity at one side produced no apparent change in the localisation of a sound when there was a phase difference at the two ears, and to assume that the effects observed in the experiment were the same as would have been observed had the intensities remained constant. Simpson (14), aware of these shortcomings, calibrated for equal intensities of the sounds at the two ears for two "frequencies, 512 and 256 d.v., by means of Rayleigh discs. No Rayleigh disc was available for the 128 frequency." But now other difficulties, of a physiological nature, arise, which I shall consider immediately. With Stewart's 'phaser' variations in the intensities of the sounds could not occur if the magnets remained at constant distances from the rotating toothed wheel. Stewart apparently considered no further refinement necessary for, from his previous investigations into the effect of binaural differences of intensity on the localisation of tones, the conclusion was "definitely drawn that intensity differences at the ears cannot be important factors in localisation over this range of frequency."

From a physiological standpoint the admitted impurity of the notes used in the experiments under discussion introduces no new factor. It may be, and probably is, a fact that however pure the tone objectively may be, it is impure by the time it reaches the cochlea. This is a matter which need not detain us here. But the intensity factor is all important, and it is the physiological aspect of this that Simpson (14) fails to consider. She apparently arranged for 'objective' equality in intensity, forgetting that the acuity of hearing may vary from ear to ear, from tone to tone and from one moment to another. It is not sufficient, I maintain, to arrange for 'objective' equality of loudness; 'subjective' equality is necessary. And especially is this necessary when the sounds are reversed, *i.e.* when the sound which at first was led to the right ear is now led to the left, and *vice-versa*. Reversal, without a readjustment of the intensities, does not suffice.

From a psychological standpoint the experiments, so far as they are qualitative, are sufficiently controlled. In the Myers-Wilson experiments, for example, the anomalous effects, which were eventually found to be due to differences of intensity, prove that suggestion did not influence the observations. But when we come to quantitative measurement, the psychological element has to be considered. I shall, later, show how largely judgments may be affected by suggestion. The only way in which reliable results can be obtained is to have observers who are in complete ignorance of what is expected, and of how far their observations agree. Very great care must be taken to ensure that the instructions given to the observers do not themselves contain suggestions. Now the only experimenters who have definitely attempted to study the phase effect quantitatively are Bowlker (3), and Stewart (16, 17). Both of these experimenters were their own observers, though Stewart incorporates in his report the corroborative results obtained for four frequencies by two other observers, apparently colleagues, who used his 'phaser.' They both (Bowlker and Stewart) had, apparently, their own theories as to the quantitative results to be expected, before they carried out their experiments. Stewart(15), had, as a matter of fact, published, some years previously, his estimate of the relation to be expected between the phase difference at the ears, the position of the sounding body, and the pitch of the note. From this standpoint, then, the quantitative results of Bowlker(3), and Stewart(16, 17), are perhaps less reliable than the quantitative results obtained by others who were studying the phase effect qualitatively.

III. THE PROBLEM.

The object of the experiments described in this paper was to obtain answers, under adequately controlled conditions, to the following questions:

1. Does a binaural difference of phase produce a change in the apparent direction of a pure tone?
2. What is the change produced by a given binaural phase difference?
3. Is the change the same for tones of various frequencies?
4. Can any general rule connecting the effects be found?

IV. APPARATUS USED.

(a) *Description.*

My apparatus(2) is essentially that used by Dr E. V. Appleton(1) of St John's College, Cambridge, in his experiments on the synchronisation of triode oscillators. It is shown diagrammatically in Figs. 1 and 2. Two arrangements of the apparatus were used. They are similar in effect except that, with the second arrangement, lower notes can be obtained.

Each oscillating circuit consisted of the following parts:

Four coils, each of about 700 turns of copper wire, wound on two rectangular frames (approximately 40 cm. × 25 cm.), two coils on each frame;

One Sullivan telephone, with shunt;

One Marconi 'R' valve;

One set of variable condensers, the largest having a capacity of 1 m.f.d. and the smallest being a variable air condenser of ·001 m.f.d. capacity;

One 6-volt lead accumulator;
One 56-volt (nominal) Siemens dry battery;
One voltmeter;
Various resistances, switches, etc., as indicated.

The telephones could be connected to either circuit by means of a 4-way switch.

In each circuit there is, when the switches are closed, an oscillating current, the frequency of which can be varied by changing the capacity of the condenser—an increase of capacity causing a lowering of the frequency. If the circuits are sufficiently close together—in my experiments the coils were about 1 metre apart—they interact. Then, if the frequency (F) of the one circuit is kept constant, and the frequency of the other, which we will assume was originally higher, is gradually lowered, rapid beats appear. The beats become slower and slower as the frequency difference is made less, till at a frequency $(F + \delta F)$ they cease and the two circuits vibrate in unison. As the frequency is lowered still further the circuits continue to vibrate in unison till, at frequency $(F - \delta F)$ they break away, and beats again appear. Now Appleton (1) has shown that, though the circuits are oscillating in unison over this range of frequencies, the oscillations vary in phase. They are 'in phase' at one end of the range, both circuits oscillating together, but there is a difference of phase of π at the other end of the range. In other words, the phase difference of the circuits varies continuously from 0° to 180°, being 90° at the middle of the range.

(*b*) *Advantages and disadvantages.*

The advantages of this apparatus for the investigation of phase effect are obvious. A note of any desired frequency can be obtained with ease and the relative phases at the ears varied at will. Also the loudness of the sounds can be adjusted for each observation by varying the shunts (*sh.*, Fig. 1) of the telephones. I consider this to be very important. But the apparatus is open to objections, the chief of which are:

(1) That the purity of the notes is not known;

(2) That, although we know that the difference between the phases of the *electrical* oscillations varies from 0 to π, we cannot be certain that the difference between the phases of the vibrations of the telephone diaphragms varies concomitantly from 0 to π;

(3) That, when the relative intensities of the sounds at the two ears are changed (by varying the shunts) other changes in the sound waves may occur, *e.g.* in the phase, or in the wave form;

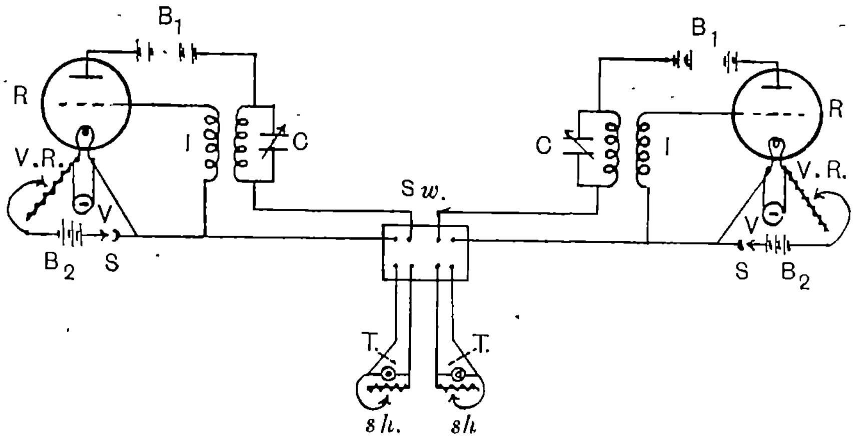


Fig. 1.

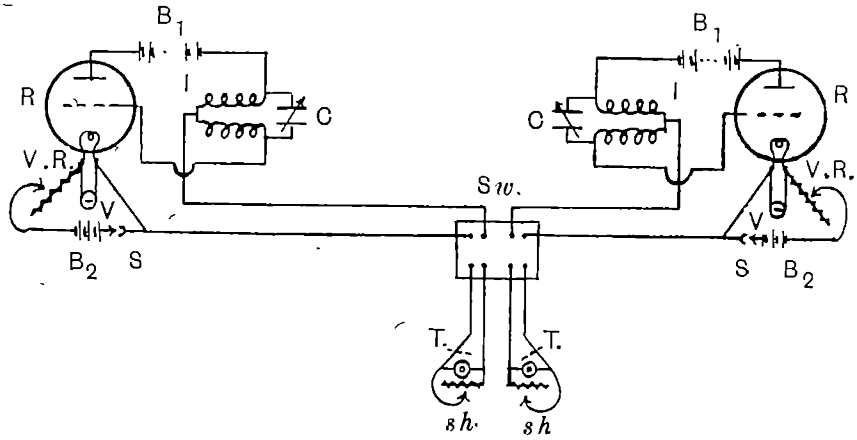
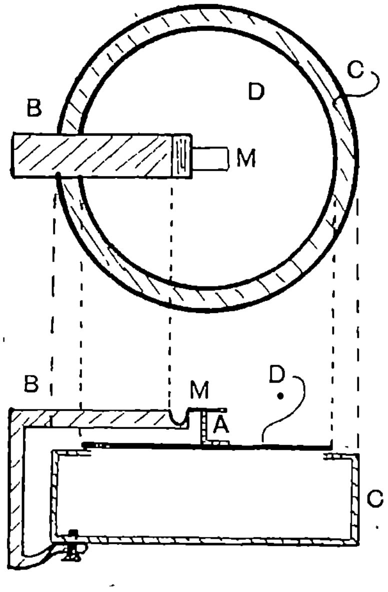


Fig. 2.



(For Figs. 1 and 2)

B_1	High Potential Battery (54 volts)
B_2	Accumulator (6 volts)
R	Valve
V	Voltmeter
S	Switch
V.R	Variable Resistance
C	Variable Condenser
I	Inductances
T	Telephone
Sw	4-way Switch, by means of which the telephones can be connected to either circuit
sh	Shunt

(For Fig. 3)

A	Aluminium Angle-piece
M	Mirror
B	Brass Arm
C	Telephone Box
D	Diaphragm

Fig. 3.

(4) That the variation in pitch throughout the range during which the circuits are vibrating in unison may cause other changes which will render the method unreliable.

(c) *Answers to possible criticisms.*

To answer the first three criticisms no theoretical replies will suffice. It is evident that irrefutable evidence can be obtained only by investigating the actual vibrations of the diaphragms themselves. I accordingly photographed the diaphragms when vibrating, mounting minute mirrors for the purpose as shown in Fig. 3. Each mirror was hinged at one end to a rigid brass arm and rested on an aluminium angle-piece fixed to the centre of the diaphragm. The mirrors[1] weighed ·000398 grm. and ·000708 grm. respectively; the aluminium angle-pieces[1] ·000598 grm. and ·000500 grm. respectively; while the weights of the diaphragms were 1·3835 grms. and 1·4895 grms. In this way the movements of the diaphragms were magnified up to 2000 times, and photographed on both moving plates and films. Pls. II, III and IV show some of the records. From them answers to the first three criticisms can be obtained.

In the first place it is clear the notes are not pure. Pure tones were not expected, since the electrical oscillations are not sinusoidal. But, as an examination of the photographs will show, the notes are not even of the same wave-form, probably because the diaphragms have not the same natural frequency of vibration. Now it was necessary that I should obtain pure tones, so I photographed the movements of the two telephone diaphragms, when vibrating under a sinusoidal E.M.F. This was done for a range of frequencies varying from 280 to 1500 d.v., and it was found that, at frequencies of about 280 to 300 d.v., the curves of both were sinusoidal. I accordingly put the telephones in the oscillating circuits between the condensers and the inductances, when they were subject to a sinusoidal E.M.F., and photographed the movements of the diaphragms again. The photographs, Pl. I, show that these tones are pure. With these tones the same phase effect is obtained as with the impure tones used. We may therefore conclude that the phase effect in localisation is not a phenomenon which requires impure tones for its manifestation.

Such an arrangement of apparatus with the telephones between the condensers and the inductances gives, on the whole, purer notes than either of the arrangements shown in Figs. 1 and 2; but this advantage is more than counterbalanced by technical difficulties. For, if the

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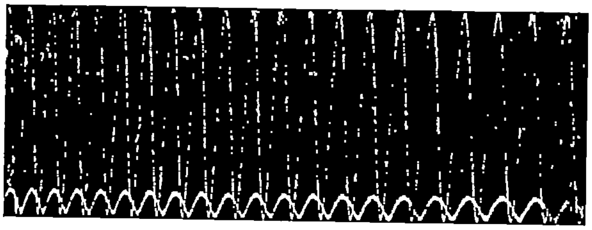
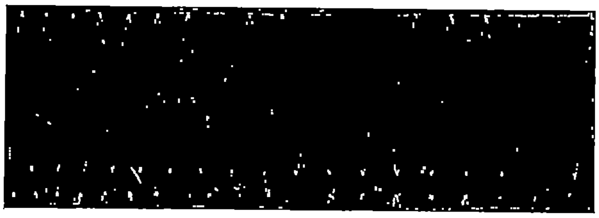
[1] I am indebted to Mr Palmer, of St John's College, for these weighments.

THE JOURNAL OF PSYCHOLOGY, VOL. XV, PART 3 PLATES I AND III



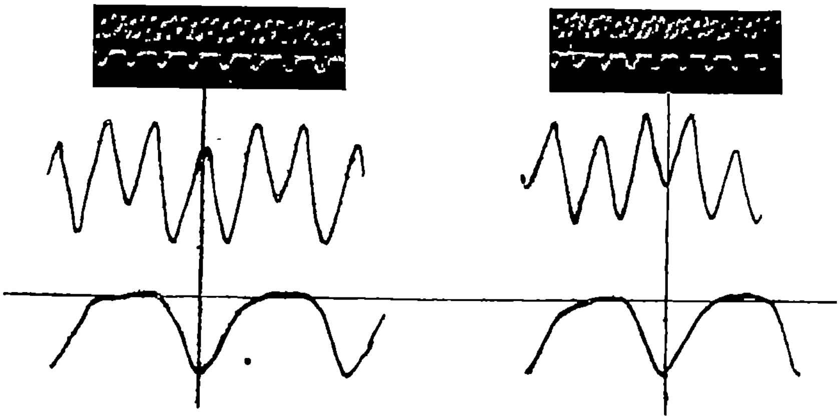
Freq. 280 d.v./sec.



Freq. 300 d.v./sec.

Plate I. Photographs showing pure tones. Magnification 2100 diameters.

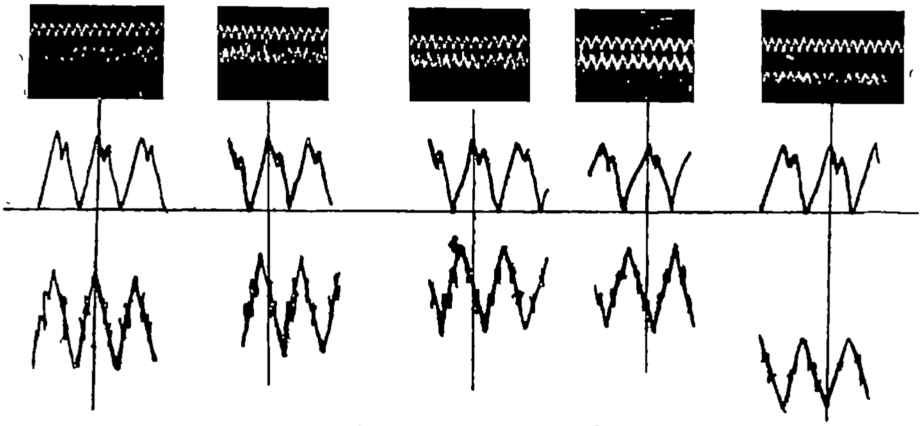
In each photograph the two curves are the tracings on a moving plate of the light reflected by the mirrors, mounted to show the vibrations of the telephone diaphragms, *vide* page and Fig. 3.



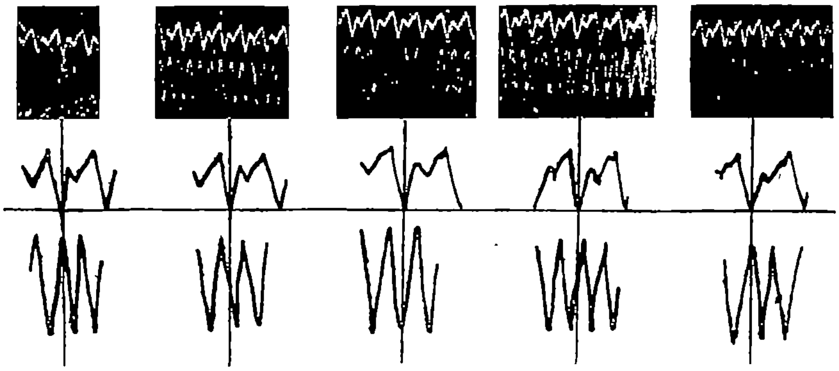
Freq. 420 d.v./sec. Magnification 750 diameters.

Plate III. Photograph to show the relative phases at the limits of the resonance range Apparatus as in Fig. 2.

BANISTER.—LOCALISATION OF TONES AT VARIOUS FREQUENCIES. (pp. 280—307.)



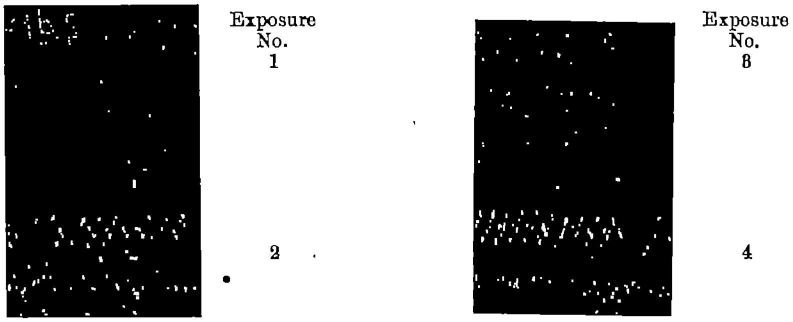
Freq. 920 d.v./sec. Magnification 1430 diameters.



Freq. 560 d.v./sec. Magnification 1430 diameters.

Plate II. Photographs to show how the relative phases of the vibrations vary through the resonance range. Apparatus as in Fig. 1.

N.B. The last photograph in the upper series was unfortunately fogged. The one here shown is a later repeat photograph to show the difference of phase of π. The adjustments were not exactly duplicated, and in consequence the amplitude of the vibration of the lower of the pair is not quite the same as that shown in the other photographs.



Freq. 540 d.v./sec. Magnification 1430 diameters.

Plate IV. Photographs to show the effect of variations in the shunt of one telephone on the vibrations of the telephone diaphragms.

In each of the above photographs the shunt to the lower telephone has been decreased after the first exposures, Nos. 1 and 3, have been made. It will be seen that 2 is similar to 1 in all respects, and that 4 is similar to 3 in all respects, excepting only that the amplitudes of the vibrations of the diaphragm of the lower telephone in 2 and 4 are less than in 1 and 3 respectively.

BANISTER.—Localisation of Tones at various Frequencies. (pp. 280—307.)

telephone is in the oscillating circuit, between the inductance and the condenser, any change in the shunt of the telephone, to adjust the loudness of the sound, will alter the capacity of the circuit, and, in consequence, the frequency of the oscillations and the range throughout which the circuits oscillate in unison; so that for every adjustment of intensity by the observer, which is in practice needed for almost every observation, the resonance range would have to be redetermined. Since it was demonstrated (p. 290) that the difference between pure and impure tones is not important for the purpose of the present experiment, the more convenient arrangement, technically, was employed.

A further criticism of the apparatus, which might be raised, is that the different complexities of the tones at the two ears vitiates the results. Now if the differences of timbre at the ears have any effect, it will be manifested as some constant change when the telephones are interchanged. I have examined the various reports carefully for such evidence; none is forthcoming. We may conclude, therefore, that, though the tones were not pure, their impurity in no way affects the investigation.

Photographs, Pls. II and III, sufficiently well answer the criticism that the telephone diaphragms may not follow the phase changes of the circuits. To make the answers clearer I have enlarged the photographs by the help of a projection lantern, and show tracings of the enlargements by the side of the photographs. It will be seen that the waves are in phase in the first photographs, and that the lower of the two waves gradually advances relatively to the other till, in the last photograph of each series, the difference of phase is exactly one-half of a wave-length, or 180°.

The photographs of Pl. IV demonstrate that a variation in the shunts on the telephones changes the amplitude of the vibrations of the diaphragms but produces no other change, and so disposes of the third criticism, *vide* p. 288.

It is more difficult to meet the last criticism, that the variation in pitch may mar the method. The only answer I can give is that, if the circuits are not too closely linked, so that the resonance range is not too wide, the difference of pitch is so slight as to be inappreciable, even to a very highly trained ear. It is less than the variation in the pitch of a tuning fork as its vibrations die away. Also, as a reference to the photographs will show, it is the change of phase that is different at the limits of resonance—the form of the individual vibrations does not vary. And yet this is not strictly true. For frequencies 1040 and

1345 d.v. per sec., anomalous results were obtained. Observers reported, "It was more a swelling, an increase of intensity, than a movement"; "Sound very diffuse and difficult to locate"; "A change of intensity only"; etc. When a movement was noted it was, as often as not, in the opposite direction to that expected. No explanation was forthcoming till the photographs were taken. These show that the natural frequencies of the telephones are about 1060 and 1350 d.v. per sec. respectively. As these frequencies are approached there is a rapid increase in the amplitude of the vibrations, and the resulting changes in loudness are sufficient to mask the phase effect almost entirely. The observations at these frequencies have, in consequence, been omitted in the following pages. It would seem therefore that, for the purpose of this investigation, the last criticism has little weight. This inquiry demonstrates the great importance of a careful physical survey of all apparatus used in auditory experiments. No one who has previously experimented on the phase effect has, so far as I know, examined his apparatus in this way. Without such an examination it is clear that the quantitative measurements obtained must be subject to much doubt.

(*d*) *Experimental procedure.*

A considerable amount of preliminary work was done before the method of experiment finally adopted was decided upon. This preliminary work enabled me to perfect my technique, and gave my observers practice in observation. My observers were not, however, given any training in localisation. I wish to emphasise this point. Also no suggestion was made that the sounds should be localised outside or inside the head; or that they should move in any special plane, either vertical or horizontal or between the two; or, in fact, that they should move at all. And no analysis of the results was made till the whole series had been completed, thus lessening still further the possibility of suggestions being received from me.

In the final experiments the observer was seated in a sound-proof room; the experimenter in an annexe. Communication between the two was maintained by electric signals, though a speaking-tube was also installed for use in emergency. The whole of the apparatus was in the annexe, excepting the telephones and their shunts.

The observer sat at a table on which were a form for his report, and the variable resistances—the telephone shunts—by which he adjusted the loudness of the sounds. On the wall in front of him, and also on the table, were diagrams showing semicircles divided in degrees to assist him in deciding the direction, in degrees from the sagittal plane, from which the sound appeared to come.

The following was the procedure. While the observer was adjusting the head-piece with the two telephones to his satisfaction, the experimenter tuned the two circuits. To enable this to be done an additional coil of wire, without condenser, was placed midway between the coils of the two oscillating circuits and in series with a telephone, crystal rectifier, and galvanometer. An alternating E.M.F., which is a maximum when the currents in the two main circuits are in phase and a minimum when their phases differ by π, is induced in this additional circuit; so the experimenter can tell, either by listening to the telephone or by watching the galvanometer, when the main circuits are tuned and when they are vibrating in phase.

When everything was arranged satisfactorily the observer signalled 'Ready,' and the experimenter switched on the sounds 'in phase.' The observer then adjusted the shunts of the telephones till the sound appeared to be in the median plane, and then signalled 'Centre,' when the experimenter switched the sounds off. After a short interval, the observer signalled 'Ready,' and the sounds, still 'in phase,' were switched on. The observer ascertained that the sound was still central —if it were not in the centre he made any necessary readjustment— signalled 'Centre,' and the experimenter, by altering the capacity of one of the condensers, produced a difference of phase of $\pi/2$ in the sounds at the observer's ears. The sounds were continued for about two seconds, and switched off. The observer wrote down his observations, signalled 'Ready,' and the process was repeated. After 20 such observations a short interval of rest was taken, the telephones were reversed, and the second half of the sitting was proceeded with. The time taken for a sitting of 40 observations was about 45 minutes.

This method of experimenting was decided on because my observers said they found it easier to judge a 'movement' than a stationary position; *i.e.* they found it easier to say the sound started from the centre and moved through a certain angle to the right or left, than to say simply that the sound was located at so many degrees right (or left). Also, by centering the sound, if necessary, for each observation, any difference in acuity between the two ears was compensated for. This is, I think, important.

The instructions given to the observers were:

(1) when ready, signal 'Ready';

(2) centre the sound when switched on; then

(3) signal 'Centre';
(4) observe what occurs;
(5) write it down; and then
(6) signal 'Ready' again and repeat.

The initial sound of each observation was 'in phase' at the two ears; and, except when 'catches' were given, the change of phase was always to a difference of $\pi/2$, but this was not known by the observer. By means of the switch (Fig. 1, *Sw.*), the sound was made to lead in phase at either ear at random.

After each sitting the pitch of the note was determined by the aid of a monochord, the possible error being less than 1 per cent.

Seven observers, four men and three women, took part in the final experiments. They were all, with one exception, under 40 years of age; and, again with one exception, all were working in this laboratory. Only one (*C*) had done work on sound localisation previously.

V. EXPERIMENTAL RESULTS.

(*a*) *Variations in the report of the individual.*

The reports show striking individual differences. I expected differences; but was scarcely prepared to find that the judgments by the same individual of the amount of the change of direction produced by the same phase change, under conditions as similar as was possible, would show such variation[1]. In only one instance—(*G*) freq. 133 d.v.—were the deviations judged to be uniformly equal in amount (90°). It was not unusual for a series of readings such as the following to be reported:

TABLE I.

Observer D.

		Deviation																		Av. deviation
Pitch		90°	70°	65°	60°	55°	50°	45°	40°	35°	30°	25°	20°	15°	10°	7°	x°	Nil	Total	
771	Left	2	.	.	.	.	.	6	.	1	.	5	1	1	1	1	.	.	18	37
	Right	1	1	1	2/1*	.	3	7	.	2	2	.	.	.	.	.	1	.	20/1	50
812	Left	3	.	2	5	.	1	4	.	.	2	2	/1*	.	.	.	1	1	20/2	55
	Right	1	.	.	2	1	1	7	1	1	3	.	.	.	1	.	.	.	18	45

* The figures under the (/) indicate that these moves of 60° and 20° were reported as being to the Right and Left respectively, whereas they should have been in the opposite directions.

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[1] It may be thought that the experimental method would induce these variations—that the observer would not 'expect' to be given the same stimulus each time—thus his judgments would, perhaps unwittingly, be influenced. The effect of giving observers random phase-differences greater than, equal to, and less than $\pi/2$ was tried, but it did not lead to any greater constancy in the judgments of the amount of deviation caused by a phase-difference of $\pi/2$.

What measure of deviation are we to take as the true one? I have examined all the reports, but have not been able to find any correction which could be applied even to the individual observer's judgments, much less one which could be used generally. For example *D*'s report for freq. 771 d.v. was:

TABLE II.

Expected direction of move	Move observed (in degrees)		Expected direction of move	Move observed (in degrees)		Remarks by observer
	Left	Right		Left	Right	
Left	45	.	Right	.	30	
,,	45	.	,,	.	30	
,,	45	.	,,	.	45	
Right	.	90	,,	.	45	
,,	.	60	,,	.	70	
Right	.	45	Right	.	45	
Left	25	.	Left	25[1]	.	
,,	35	.	,,	45	.	
,,	90	.	,,	25	.	
Right	.	x*		.	.	* Am not sure of amount of move, but it was to the right.
Right	.	45	Left	7†	.	† This may have been to the right.
,,	.	60	,,	15	.	
Left	.	60	Right	.	45	
,,	90	.	,,	.	50	
,,	25	.	,,	.	45	
Right	.	35	Right	.	35	
,,	.	65	Left	10	.	
,,	.	50	,,	20	.	
Left	25	.	,,	45	.	
,,	45	.	Right	.	50	

[1] The observer came out of the room after having had only 39 observations, saying he had had 40. It was found he had missed one space here.

It will be seen that the judgments of the amounts of the movements not only vary, but that the variation is irregular. In some cases the first of a series of movements to one side is the greatest; in others the maximum is the middle of the series; in others again the last one has the greatest value. It is however to be noted that the only mistakes—No. 10, when the amount of the movement is not known; and No. 13, when the direction is wrong—occur when the direction is changed.

(*b*) *Variations probably due to Psychological Factors.*

This variation in judgment is different from the 'inertia of judgment,' noted by Myers and Wilson (18), which occurs when a judgment is made of the position of the sound with a certain phase-difference, followed by another judgment with another phase-difference, and so on. The variation may, I think, be ascribed in part to the effect of witting or unwitting expectation. It will be noticed that three movements were given to the left and three to the right alternately during the first part of this sitting. It is probable that such regularity was, consciously or unconsciously, not expected, and the consequence was confusion and a wrong judgment. Many times observers reported that, through expectation not being fulfilled, they were not able to make any judgment —they knew the sound had moved, but could neither say whether it had gone to the right or to the left, nor how far it had gone.

Various factors appeared to give rise to expectation. The run of previous movements was perhaps one of the most common, though observers endeavoured to make each judgment on its own merits. Sometimes the sound, when switched on, appeared to come in on one side and to move by itself to the centre; at others it appeared to remain to the right or left of the centre, so that the shunts to the telephones had to be changed to 'centre' it: both of these phenomena gave rise to expectation. (*C*), for instance, said that readjustments often made him anticipate the direction of the movement till he found, by experience, that this indication was not to be trusted. One of his remarks was "Movement lost owing to anticipation. Sound came in on one side, I forget which, and I expected it to move to the opposite side. When it moved I could not tell where it went." Other observers also made similar remarks.

The effect of suggestion is well brought out in the following:

(*F*) had just finished a sitting and, instead of reporting 20 movements to the right and 20 to the left as given her, she reported 9 right and 31 left. (*B*) followed and, at the close of her first 20 observations, I saw at a glance that she had the direction of every movement, except one which she had not detected, correct. The following conversation followed:

Exp. "You are most consistent."

(*B*) "How?"

Exp. "You give the changes to right and left as I expect them."

(*B*) "What do others do?"

Exp. "They sometimes give them all to one side."

In the second half of her sitting, which should have given 11 to the right and 9 to the left, she reported 18 to the right, and 2 'No movement.' She perhaps thought I intended to give her a series of movements in one direction, and that I wished to see if I could influence her so as to make her report movements to either side. I repeated the frequency the next time she came, when she made only two mistakes in direction. These 'second sitting' figures are the ones included in the statement which follows.

We see therefore what a large part expectation and suggestion may play. It would be very interesting to carry out experiments, under properly controlled conditions, to see whether movements of the sound could be produced by specific suggestion made for a definite purpose, and whether it would be possible, by this means, to obtain data which would substantiate all the different theories of the phase effect.

In this particular series of experiments, the most general effect of the psychological factors seemed to be to minimise the amount of the movement, and more especially to cause the movement to be missed altogether, so that the 'average,' or arithmetic mean, of the movements would tend to be weighted on the smaller side by an amount which cannot be estimated. It is for this reason chiefly that I have felt constrained to take the 'mode,' the most frequent movement produced by the difference of phase, as the measure of the phase effect.

(c) *Reports of observers and their differences.*

Table III shows the modes of the observations of five of my observers for various frequencies:

Various individual differences will be noticed. Three observers, *A*, *D* and *E*, obtain movements of 90° for low frequencies; the other two, *B* and *C*, have maximum movements of 45° and 70°. Also *A* and *B* record practically constant movements, 90° and 45° respectively, whatever the pitch of the note; while *C* and *D* record smaller movements for higher frequencies than for lower ones. *E*, on the other hand, appears to perceive a smaller movement with rise of pitch.

But my other observers show greater differences still. The modes of *G*'s observations are shown in Table IV.

For those frequencies for which the phase effect is easily discernible, the movements to the left are constant at 90°. But to the right they vary: 90° for a frequency of 133 d.v., an octave higher dropping to 10°, thereafter increasing as the pitch is raised. It is unfortunate that this observer has left the country, so that further experiment with him is not possible.

Observer *F* is of a different type. She has a great tendency to localise the fused sound, which results when two sounds, of the same

TABLE III.

Frequency d.v.	Observers *A*	*B*	*C* 1st series	*C* 2nd series	*D* 1st series	*D* 2nd series	*E*
	°	°	°	°	°	°	°
133	90	45[1]	.	.	90	.	.
250	.	.	.	70	.	90	90
278	.	45	60	.	25[2][f]	.	.
339	.	45[1]	45	.	45	.	.
343	.	.	.	70	.	45	(70)
540	.	.	.	45	.	(45)	(45)
559	.	30[b]	45	.	45	.	.
658	90	.	.	.	.	.	.
740	.	.	.	45	.	(45)	70
771	.	30[c]	45	.	45	.	.
812	60[a]	30[d]	45	.	45	.	.
845	.	.	.	45	.	45	45
854	.	45[1]	.	.	.	.	.
865	.	.	.	.	45[2][g]	.	.
1000	90	45[e]	45	.	(45)	.	.
1120	.	.	.	×	.	45	×
1161	90	(30)[1]	.	.	.	.	.
1365	(90)	.	.	.	.	.	.
1377	.	.	.	45	.	45	(25)
1583	×	×	.	.	.	.	.
1705	.	.	.	(45)	.	(45)	×

Remarks

[a] This was the observer's first attempt. She said at the time that she had not really grasped the meaning of the measure and that the amounts of the movements should not be considered accurate.

[b] The most frequent observations were 10 (45°), 26 (30°).

[c] The most frequent observations were 11 (45°), 23 (30°).

[d] The most frequent observations were 15 (45°), 17 (30°).

[e] This figure is the mode of the 'second sitting' report, *vide* previous page.

[f] The mode of 20 observations which were 1 (90°), 3 (60°), 3 (45°), 3 (30°), 4 (25°), 2 (20°), 2 (15°), 2 (0°).

[g] The 20 observations were 5 (60°), 1 (45°), 1 (40°), 1 (35°), 5 (30°), 1 (20°) 2 (nil), and four the directions of which were misjudged.

× indicates more than 15 mistakes in the 40 judgments of the direction in which the sound moved. A judgment was counted wrong if no movement at all was heard, or if the direction was judged wrongly.

( ) indicates 11 to 15 mistakes in the 40 judgments of the direction in which the sound moved. A judgment was counted wrong if no movement at all was heard, or if the direction was judged wrongly.

N.B. The chances of 30/40 *guesses* of direction being correct are less than 1 in a 1000.
„ 25/40 „ „ „ „ 5·7 per cent.

[1] Mode of 80 observations.

[2] Mode of 20 observations.

TABLE IV.

Frequencies	133	279	339	559	771	1000	1583
Left	90°	90°	90°	90°	(40°)	(90°)	×
Right	90°	10°	20°	30°	(40°)	(90°)	×

( ) indicates 11 to 15 mistakes. × indicates more than 15 mistakes.

pitch and with a phase difference of 90°, are led to the ears separately, on the left. The next table shows the sides towards which she judged the sounds to have gone:

TABLE V.

		Frequency d.v.							
		133	278	637	658	771	812	1000	1345
No. of movements judged to have been to the ...	Left	16	12/4	20/6	20/9	39/37	13/9	19/11	–/4
	Right	14	11	11/1	10	2/1	15	9	3/1
	?	.	2	1	.	.	.	.	.
No movement at all observed ...		.	1	1	1	1	3	1	27
Total		30	30	40	40	80	40	40	35

There there is a (/) the figures over/under the (/) indicate that on those numbers of occasions the judgments of direction were correct/incorrect.

It will be noticed that, excepting frequency 1345 when, as we have seen (p. 292) the phase effect is masked, only two movements were judged to the right in error, while 76 were localised wrongly on the left. The observer herself noticed this tendency, but did not know whether she had heard rightly or wrongly. On one occasion she remarked that she "tried at times to get the sound over to the right, but failed."

One-sided tendencies have been noted before. Myers and Wilson (18) notice such idiosyncrasies. More (9), quotes his own case. Working with tuning forks, the sounds from which were led to the ears through rubber tubes of variable lengths, More obtained curves for frequencies of 768 and 1024 d.v. which were almost entirely situated on the left—the side of his weaker ear. But there is an important difference between his results and mine. He had no means of adjusting the fused sound to a central position when the sounds at the two ears were in phase. His zero therefore was half left, and the sound travelled between the two extremes of full left and centre as the phases varied from $+\pi/2$ to $-\pi/2$. In other words the sound seemed to come from a position more to the right, or more to the left, of the zero position as the sound wave at the right, or the left, ear led in phase. But *F* started from the central position every time, and yet judged 50 per cent. of the movements which should have been to the right for frequencies 658 and 1000 d.v. as left; and, for frequency 771 d.v., judged 37 out of 40 moves as left when they were expected to have gone right. For this last frequency (771), her results were so remarkable (for she judged all the 20 movements which should have been to the right as left, and put only one, which should have been left, to the right), that I repeated the frequency when she came to me two days later. This time, of 20 movements to the right, she judged 2 correctly, put 17 to the left, and once heard no movement at all. Other observers tested with this frequency on the same days were normal. The lateral preference is not, so far as I have been able to ascertain, due to any difference in the auditory acuity of her ears.

Since the observer had no difficulty at any frequency in getting a right and left effect by altering the intensities of the sounds at the ears, it might be thought that this case is complementary to that of those observers, reported by Stewart(16, 17), who, at certain frequencies were sensible to differences of phase at the two ears but were insensible to large differences of intensity if the phases were kept constant. This may be so. At the same time an incident at one of *F*'s preliminary sittings is of interest. It was a hot day, towards the end of the morning, and the atmosphere of the sound-proof room had become very close. *F* had been making more 'mistakes' than correct reports; and then suddenly she gave 20 which, with a single exception when a change of direction was made, were correct. I asked, "Have you anything to say about the last observations?" *F*, "I don't know whether it is that this place is a bit stuffy, but I almost dozed off several times, and feel a bit sleepy. That may make my last observations not so consistent[1]."

It may be that this only shows the great effect the attitude of the observer has on the reliability of his reports. Attitude undoubtedly counts for much. Too great concentration seems to be very liable to lead to expectation, and should be avoided. One observer found that his best attitude was one of "relaxed attention, such as one adopts when one knows one will get a snap-shot at some animal but is uncertain when and where"; two of my most consistent observers listened "with closed eyes, as though listening to a concert." The fact that, under certain conditions, increased concentration of attention appears to lead to decreased accuracy of observation has frequently been noticed by other experimentalists.

VI. DISCUSSION AND COMPARISON OF THE QUANTITATIVE RESULTS WITH THOSE OBTAINED BY OTHER EXPERIMENTERS.

(a) *Bowlker's results.*

As I have already stated, two experimenters, viz. Bowlker and Stewart, have definitely investigated the problem I have endeavoured to solve. They approached the problem from the physical, rather than the psychological, side. Bowlker, who was his own observer, states (3, p. 320), with regard to his preliminary experiment with a tone of 686 d.v. in which he investigated the effect of putting tubes of various lengths over his ears, that "in order to avoid psychological errors of imagination and expectation, no guesses at what would happen were made, nor any theory formed as to the probable position of the image *till two or three complete sets of experiments had been made*" (my italics). From this it would seem that he had formed his theories before he went on to determine the phase effect for notes of various wave-lengths. His method, in making this latter enquiry, was to place tubes of equal length over his ears, to turn round slowly and to follow the apparent movement of the sound of a fixed organ pipe. The sound seemed to move in the opposite direction to that in which he turned, till a point was reached where an 'image' appeared suddenly on the other side. There was at this moment, he found, a difference of phase of π between the waves arriving at the two ears; and the angle between these two 'images,' the 'cross-over angle,' as he called it, represents the deviation to right and left caused by a phase difference of π. From his measurements, Bowlker states (3, p. 326), "it would seem that for wave-lengths below 20 inches" (freq. 654 d.v.), "the angle of displacement was very roughly proportionate to the wave-length—for a wave-length of 4·4 inches (freq. 2974 d.v.), the displacement would only be 5° or 6°." In a footnote he says this law does not hold with very short wave-lengths, down to 2 inches (freq. 6543 d.v.): the displacement for these wave-lengths is larger than the law would give. He also found, in another experiment, that a difference of 2·4 inches in the lengths of the paths to the two ears produced approximately the same deviation for wave-lengths varying from 40 to 6·4 inches (freqs. 327 and 2045 d.v.).

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[1] It would be of great interest to investigate instances of lateral preference of this description to see whether, in the case of the other senses, a similar phenomenon is exhibited, and, if so, to what extent the preference depends upon the 'attitude' of the observer.

The actual data are shown in Diagram I. For ease of comparison I have adopted the coordinates used by Stewart—the ordinate, (ϕ/θ), is obtained by dividing the phase difference (ϕ) at the ears, 180°, by the deviation observed, *i.e.* the 'cross-over' angle; the abscissa represents the frequency.

(b) *Stewart's results.*

In the same diagram I have also shown the results reported by Stewart. The points indicated by dots are those Stewart himself obtained with his 'phaser'; the crosses represent the results he obtained by the dynamic method, by noting the time when the rotating sound, produced by two tuning forks slightly out of tune, was in a certain position; the circles represent his observations with apparatus similar to that used by Myers and Wilson; and the 'deltas' and squares represent the observations of two other observers who used Stewart's phaser but modified his method. These observers did not judge the position of the fused sound when there was a given difference of phase at their ears. Instead they were provided with a mechanical contrivance by means of which they were able, from the sound proof room, to adjust the phaser till the sound appeared to be in a certain position. The experimenter then noted the phase difference. The values obtained for ϕ/θ are very consistent, and, as Stewart points out, they "correspond remarkably well" with the calculations made by himself in 1914 (15), and by Hartley in 1919 (6). Commenting on his results, Stewart remarks (16, p. 441), "The experiments of Bowlker are in close agreement with (mine). He found that a wave-length of 36 inches was the one giving a displaced image of 90° for a difference of 180°. This would be a frequency of about 380 d.v. Reference to our curve shows that the frequency in our experiments would be 352 d.v. for one observer and 440 d.v. for the other." Such a statement I consider to be misleading. Bowlker's results, as a reference to the diagram will show, are "in close agreement" with Stewart's at one frequency, indicated by the point where the two curves intersect; at other frequencies the agreement is by no means a close one. Further, I would note that Miss Simpson (14), from whose report the values indicated by the circles are taken, had four observers in all—Stewart and three others. I show the results of these other three also on the same diagram (I). And again the agreement does not appear to be very close.

(c) *Results of other experimenters.*

Let us now see what other experimenters, who have worked on the phase effect without going definitely into the present problem, have found. The chief ones are More and Fry (10), Myers and Wilson (11, 18), and More (9). Their results are shown in Diagram II.

It will be seen that their observations differ greatly from those of Bowlker and Stewart.

(d) *Results of my observers.*

The results of my five observers may now again be considered, and perhaps most conveniently by turning to the next diagram (III), where the results have been plotted, the values of ϕ/θ being obtained from Table III, by dividing the deviation by the difference of phase (90°) which produced it.

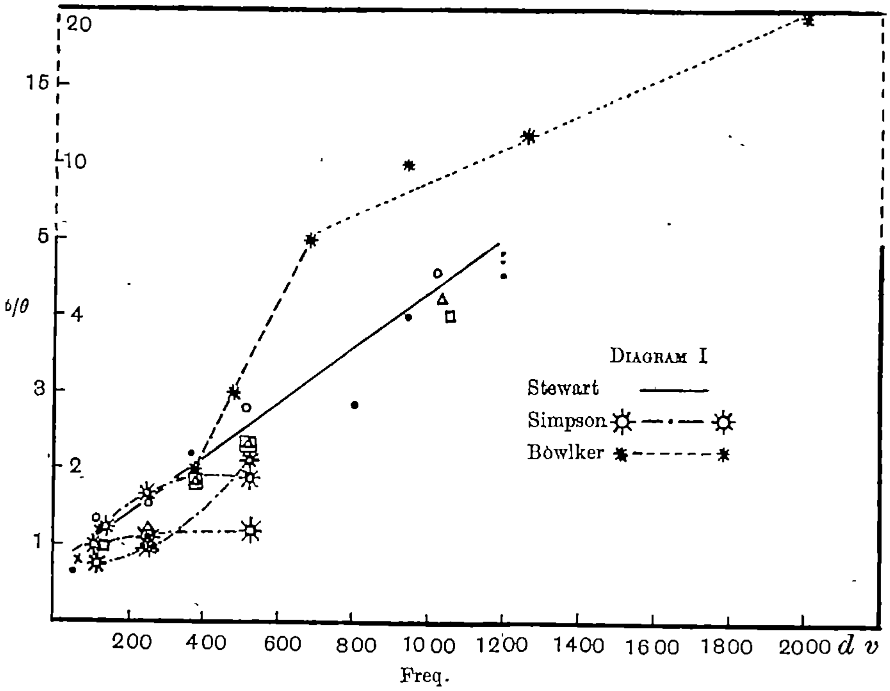

Diagram I

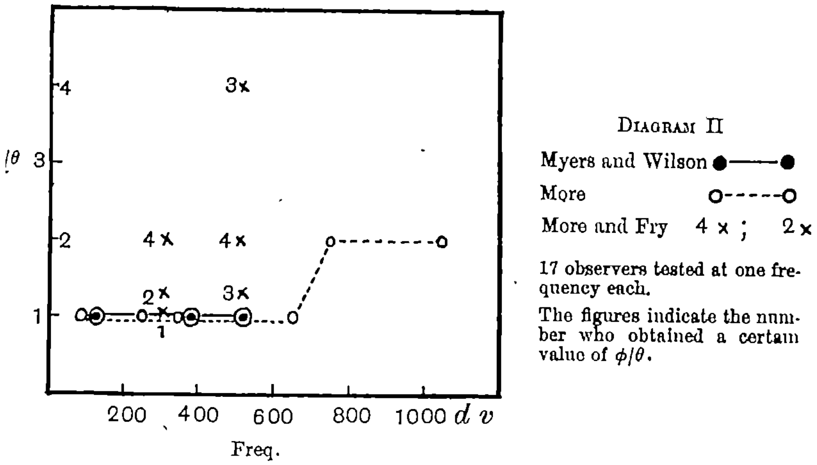

Diagram II

17 observers tested at one frequency each.

The figures indicate the number who obtained a certain value of ϕ/θ.

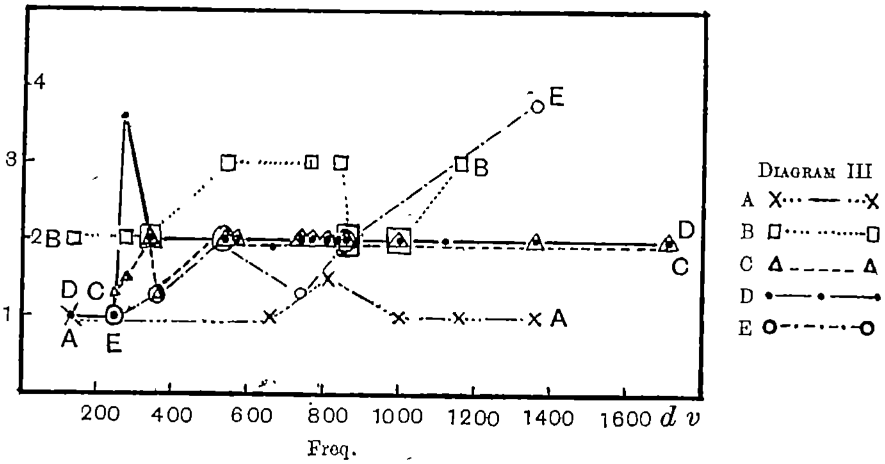

Diagram III

The first fact to be noticed is that no two observers agree. Individually, however, each observer is consistent; for, within certain ranges of frequency, the value of ϕ/θ, obtained from the observations of any particular observer, is constant. (*A*) is the most consistent. For all frequencies, except 812 d.v., the value of ϕ/θ calculated from her observations is 1·0. She is the only observer who had no previous practice; and this frequency, 812 d.v., was given to her at her first sitting. It seems possible, therefore, that the remark she made at the end of the sitting, viz. that this was her first sitting and she found it difficult to estimate the amount of the deviation in degrees, may explain the discrepancy.

For (*D*) and (*C*) the value of ϕ/θ is 1·0 and 1·3 for low frequencies, and 2·0 for higher ones. (*D*) has one abnormal value, 3·6, at frequency 278 d.v. On this day (*D*) had to leave early to fulfil an important engagement, so there was time for only 20 observations. These were 1, (90°); 3, (60°); 3, (45°); 3, (30°); 4, (25°); 2, (20°); 2, (15°); 2, (0°), and show a rather unusually large amount of 'scatter.'

(*B*) comes next. For her the value of ϕ/θ varies between 2·0 and 3·0 without any apparent reason. In other words a phase difference of 90° between the sound waves at the two ears produces a deviation, which may be one of 45° or of 30°; but which of these deviations shall be produced does not appear to depend on the frequency alone.

(*E*), however, differs from all. He appears to give results which are similar in nature to those of Bowlker and of Stewart. Perhaps he is a more accurate observer than the others, and perhaps his results are such as would be given by the normal person. But a reference to Table III shows that he made more mistakes than any one in his judgments of the directions of the movements. Some other cause must therefore be sought; and perhaps it is to be found in 'expectation' or 'suggestion,' the great influence of which has already been emphasised. (*E*) is myself. I had read most of the literature bearing on the experiment during the course of the investigation, and was very impressed with Stewart's paper (16), which seemed to be sound both theoretically and experimentally. There is no doubt that I 'expected' the same difference of phase to cause a deviation of decreasing magnitude as the pitch of the note was raised. I endeavoured to judge each movement on its own merits, and not to let any preconceptions influence my judgments: yet I could not help knowing roughly what the frequency was, and I think it very probable that my judgments were biassed accordingly.

(e) *Conclusions.*

These experiments show that the normal person can appreciate a difference of phase at his ears, and that, for frequencies up to about 1400 d.v., the limit depending on the individual, this appreciation enables him to judge that the sound comes from a rather ill-defined direction on his right or on his left. The fact that some observers obtain larger movements for low frequencies than for high, might be regarded as supporting Bowlker's and Stewart's finding which, broadly speaking, is that the amount of the deviation is proportional to the time difference between the arrival of similar parts of the sound wave at the ears[1].

This conclusion is, I would remark, in general accord with that reached by v. Hornbostel and Wertheimer (7, 8), for the perception of the direction from which a noise appears to come. These experimenters found (*a*) that, if two telephone clicks are led, one to each ear, only one click is heard, provided the time difference between the arrival of the sounds at the ears is not too great; (*b*) that this fused click is localised on the side of the ear at which the sound arrives first; and (*c*) that the angle between the apparent direction of the fused click and the sagittal plane is proportional to the time difference at the two ears. I have no reason either to believe or to doubt these results for noises[2], but I do doubt if any observer, who had no preconceived theory, and who had not undergone careful training in the actual appreciation of phase differences in accordance with the theory, would yield any close confirmation of Bowlker's and of Stewart's results.

VII. FURTHER PROBLEMS.

This enquiry leaves many of the problems connected with the phase effect unanswered. In particular it has not directly touched the main problem of the mechanism of the phase effect.

No attempt has been made to throw any light on the theory, which is maintained by Myers and Wilson and by many physiologists, that the phase effect is, on analysis, an intensity, or other effect, due to bone conduction of the sounds from ear to ear.

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[1] I would note that Stewart does not suggest that our judgment of direction is based on the appreciation of these time differences. He considers it "necessary to admit direct perception of phase, at least until a possible different interpretation of the effect of phase is secured."

[2] I would point out that Dunlap (4) found that observers localised noises "in a preferential way," judging them to come from a direction making a certain angle with the sagittal plane, whatever "the actual position of the sound." He also found that "the preferred position is not determined by the character of the sound, or by the environment."

It would, however, seem that the 'time-difference' theory—that the ear appreciates the difference of phase as a difference between the times of the arrival of similar portions of the sound waves at the two ears—is not strictly true, for we should have to suppose that the variations in the time differences which accompany the rise in pitch are compensated for, in some manner, when the actual judgment of the direction from which the sound appears to come is made.

The problem of the nature of the phase effect is now being investigated.

VIII. SUMMARY.

In these experiments an investigation into the effect of binaural phase differences on the localisation of tones has been carried out.

An attempt has been made to eliminate the possible sources of error in previous investigations by using more adequate controls, *e.g.* resonance effects have been eliminated; the sounds have been adjusted for subjective equality of loudness for each observation; suggestion has been minimised. In particular the actual objective characters of the sounds used have been carefully investigated—a matter of great importance if the results are to be of permanent value.

The conclusions arrived at may be briefly summarised as follows:

1. Pure tones can be localised by binaural differences of phase.

2. Impurities[1] in the tone, and binaural differences of timbre, do not, in general, introduce disturbing factors in such localisation.

3. The limits of frequency, at which binaural differences of phase cease to be appreciated, vary with the individual. The phase effect has been observed with frequencies ranging from 133 d.v./sec. to 1705 d.v./sec.

4. A given binaural difference of phase makes the sound appear to come from a certain, rather ill-defined, direction on the right or on the left. The angle between this direction and the sagittal plane varies with the individual; but it is constant, for the individual, over certain ranges of frequency.

IX. ACKNOWLEDGMENTS.

Finally I wish gratefully to acknowledge the assistance I have received, particularly from

Dr Appleton, who first suggested the use of this apparatus. Without his assistance these experiments would scarcely have been possible.

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[1] No investigation has been made into the limits, if any, to the impurities which can be introduced without interfering with the 'phase effect.'

Mr Bartlett, the Director of the Laboratory, and Dr Hartridge, who have always been ready to help with suggestions, advice and criticism.

Dr Kapitza, who suggested the method of photographing the movements of the diaphragms of the telephones. But for these photographs, which were taken in the Physiology Laboratory Lecture Theatre, by the kind permission of Professor Langley, the investigation would have been robbed of the greater part of its value.

Miss Eckhard, Miss Vernon, Mr Mears, Mr Pyke, Mr Sillitoe, and my wife, who so kindly acted as observers.

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PUBLICATIONS RECENTLY RECEIVED

Hearing. By Prof. R. M. OGDEN. London: Jonathan Cape. 1924. xiii + 351. Price 15*s.*

This volume, it is stated in the Preface, is a by-product of reviews and critical studies of the psychology of hearing. It was at first intended that the book should be a brief summary, more or less popular in nature, of the present state of our knowledge of hearing; but, as the work went on, the author was led to amplify his material and to employ a somewhat condensed treatment of many details. The result is, frequently, inappropriate terminology; for example, the perilymph and endolymph of the semi-circular canals are called 'water' on p. 24. Such nomenclature may be proper in the popular article: it is scarcely suited to a scientific treatise. There are many similar lapses; but one mistake, which should not have been allowed to stand, occurs on p. 28, where it is stated that the liquid in the labyrinth is not elastic—the liquid *is* elastic, it is *very* elastic; but it is not *compressible.* Another phrase, which seems to me fundamentally wrong, appears on p. 11: "the wave-length may be modified without altering the frequency." What may be modified without altering the frequency is the wave-form, not the wave-length. This would appear to be what is intended for, on p. 12, it is stated that "when the trough is lengthened a brighter tone is produced, and when it is shortened a duller tone results." I would have been satisfied that this was an oversight were it not for the number of times 'variation in wave-length' is used for what I would call 'variation in wave-form.' But perhaps there is something more subtle in this 'variation of wave-length' than I have been able to grasp.

Separate chapters are devoted to the physical basis of sound; the physiology of the ear; the pathology of hearing; tones; vocables; noise; tonal-fusion; the character of sounds; the musical scale; language; music; the localisation of sound; and aural education. In these chapters most of the recent work is reviewed, the divergences between the views are pointed out, and some of the more pressing problems are stated. The best chapters are those dealing with the attributes of sounds and the effects of the various integrations of these attributes, chapters III to VII. On these topics the author has his own views; but at the same time he states the experimental facts, and also the opinions of others, very fairly. I would, however, like to see further experiments performed with the object of discovering the factors which condition the volume of a sound, since 'volume' plays such a large part in the various hypotheses put forward; for example, on page 150 it is stated "Let us first note that tonality as well as fusion rests upon the sense of interval; and interval, as we have seen, is limited by our ability to discriminate a difference of volume." And, on page 67, we read "Physically regarded, it is the vibrational frequency that appears to condition variations in volume, just as it also conditions, though in a different way, variations of pitch." Now, physically regarded, it seems possible that other factors may also condition variations in volume. Halverson, quoted on page 266, says, when describing experiments on sound localisation, that the lateral image "appears to be auditorily more...voluminous...than when frontally perceived." Other experimenters, I would remark, find that the lateral image is the more compact. But, whether the lateral image or the frontal image is the more voluminous, it is evident that the volume may vary while the frequency remains constant.

On page 269, when discussing the theory of binaural localisation, Prof. Ogden states that no "attribute of sound has been discovered which varies directly with phase." This is literally true, if the sentence means it has not been *proved* that any one of the attributes of sound, which he allows to be attributes, varies directly with phase. But if, instead, the meaning of the sentence is that no hypothesis has been put forward, explaining the phase effect in terms of one of the attributes he allows, the statement is wrong; for Myers and Wilson proposed, some sixteen years ago, that the phase effect could be explained as an intensity effect due to bone conduction, and showed how the intensity might vary concomitantly with phase.

In spite of all this criticism, the book can be recommended to all who are interested in hearing. It will be appreciated by the musician, who will find his problems regarded from a new standpoint; by the teacher of music, if he is also a student; and by the experimental psychologist, who will find in its pages a lucid and connected account of present-day theories and hypotheses.

H. B.

Anthropology and Psychology: a Study of some Points of Contact. By Prof. C. G. Seligman. Presidential Address to the Royal Anthropological Institute of Great Britain and Ireland. London. 1924.

Professor Seligman finds his starting point in Jung's well-known distinction between extravert and introvert types. He describes these types, discusses, with illustrations, their appearance in the fields of art, literature and, very briefly, in science. He speculates concerning their biological character, suggesting, on admittedly insufficient evidence, that these 'types' are inherited and that the inheritance correlates with inherited physical characters. He considers it probable that the extravert type is more common among the less civilised peoples, and briefly discusses the distribution of the types among the Nordic, Alpine and Mediterranean races. He then turns to a study of dreams of non-European peoples, showing that the dream mechanism appears to be much the same in less developed as in more developed groups, and finally he demonstrates that dream interpretation among primitive folk has brought out some of the very principles employed in recent psycho-analytic theory, and that there is considerable evidence that "dreams with the same manifest content to which identical (latent) meanings are attached occur, not only in cognate groups, but also among peoples of diverse race and in every stage of culture."

The whole address bristles with points of profound psychological interest. Jung's twofold division by its very facility of application seems to many students to be not only a simplification, but a misleading simplification of the facts of human character. It can scarcely be maintained that Prof. Seligman does anything to dispel this suspicion; nevertheless, many of the points which he makes in the course of his discussion are well worth following up, particularly those in the biological section. Beyond doubt, however, the best part of the address deals with the dreams of primitive people. The examples are interesting, the analogies of savage and civilised interpretations astonishing, and the bearing of the actual evidence of 'type dreams' on the great question of 'universal symbolism,' is a matter of first-rate importance. To all anthropological psychologists and to all psychological anthropologists—and there are growing numbers of both—Prof. Seligman's address may be most warmly commended.

On page 15 Henry James is credited with a well-known remark made by his brother William.

F. C. B.

Social Aspects of Psycho-Analysis. Edited by ERNEST JONES, M.D. London: Williams and Norgate. 1924. Pp. 240. Price 7*s.* 6*d.* net.

This book is an expansion of lectures delivered by eminent Freudians at the request and under the auspices of the Sociological Society. The lectures are concerned with the application of psycho-analysis to present-day social problems such as Education, Politics, Vocation and the Family. Dr Glover contributes a section on *Man the Individual* and Dr Ernest Jones has an introductory lecture on the *Relation of Psycho-Analysis to Sociology.* The book will serve two useful purposes: it points out that where the older psychologists and educationalists were lacking was in ignoring what may be called the intimacies of life—a deficiency which psycho-analysts set out to supply in some earnest; secondly, it is clearly shown that in dealing with individual difficulties psycho-analysts are really grappling with social problems. In the words of Dr Jones, "the neuroses are themselves not so much diseases in the ordinary sense as forms of individual reaction to social situations, problems and difficulties." Here are two capital starting-points for a future social psychology and the book will be found highly stimulating and suggestive. Perhaps the best contribution is that by Barbara Low on *Education*, in which she urges the importance of recognising and if possible sublimating the rich phantasy-life of the child. How to set about this task, and the finding of the material and methods best suited to aid such sublimation, is the problem before the teacher-psychologist. M. H.

Fundamentals of Vocational Psychology. By CHARLES H. GRIFFITHS. New York: The Macmillan Company. 1924. Pp. xiii + 372. Price 12*s.* net.

This book is concerned more with the theory of Vocational Psychology for students of the subject than with its practical application for use by employment managers and vocational counsellors. The author gives an interesting account of the historical approach to the subject by way of the various systems of physiognomy, and stresses the importance of the interview in selection from among the applicants. It seems, however, that we are still very far from any standardised system of vocational testing. Some emphasis is laid upon the fact that the employment manager can make only a very rough and ready selection by testing; while the vocational counsellor is advised to do little more than give information, and to discourage those whose mental or physical powers are quite inadequate for the occupation they propose to take up. Until more work has been done in the preparation of specific occupational tests providing a definite standard for success and failure, the subject will retain the rather academic aspect presented in this book. M. V.

The Teaching of Reading. By H. G. WHEAT. New York: Ginn & Co. 1923. Pp. 342.

This is an exasperating book, as its faults could easily have been avoided. Its general plan is good, and it contains an exposition and discussion of almost every point that a teacher of reading in schools could require, each being accompanied by practical suggestions, some in great detail. But the book is repetitive and top-heavy —its headings, introductory remarks, and summaries are excessive in proportion to the topics themselves, and, though as a text-book it makes no claim to be original, an average of one page of quotations to six or seven of text is irritating and superfluous.

The first half gives a clear orthodox account of the physiology and psychology of the process of reading, though some of the psychology is stated perhaps a little too dogmatically, for our knowledge on this subject is still uncertain and incomplete. In the second half two problems important for the teacher are dealt with separately—the best age at which to start teaching to read, and how to cope with the backward pupil. The former discussion however offers little positive argument in support of the view that it should be not later than six years, but the latter topic is treated excellently and in detail.

The mistakes of old-fashioned methods of teaching, their failure to distinguish between the processes of oral and silent reading, and how these ought to be related, are all well treated. The author shows how the fast reader is likely to read more, and not less intelligently than the slow reader, which is important in its bearing on the first principles of teaching, and on the child's future intellectual life. He rightly stresses, too, the importance of meaning rather than expression and pronunciation; hence the actual content of Reading Primers should be chosen carefully and with the object of interesting the beginner. But some of the examples offered are likely to send shudders through any Freudian who correlates his views with the practical details of the education of the very young.

There is a good bibliography.

R. L. P.

The New Examiner. By Philip Boswood Ballard, M.A., D.Litt. London: Hodder and Stoughton, Ltd. 1923. Pp. 269. Price 6*s*. net.

This book puts forward an interesting plea for the new method of examination, that is, by means of a number of short standardised questions, answered by a word or a mark, which can be quickly and accurately corrected by the examiner; and contrasts it favourably with the old method, to wit, by the essay question, which tests a variety of attainments together, and is thus difficult to mark with accuracy and consistency. The objections, both philosophical and practical, to the new examination are dealt with—though it cannot be said that the former are all completely overcome. A series of new tests for school-children is given in English Literature, Mathematics, History and Geography, and a series of group intelligence tests for young children, together with their corresponding norms. This book provides a welcome relief from the many somewhat pedantic utterances on this subject; and should be welcomed alike by examiner and examinee.

The Function of Statistical Method in Scientific Investigation. By G. Udny Yule, C.B.E., M.A., F.R.S. Industrial Fatigue Research Board Report, No. 28. London. 1924. Pp. iv + 14. Price 6*d*. net.

In this report, the subject-matter of which was delivered as a lecture at the University of Leeds in June 1923, Mr Yule stresses the importance of statistical method to investigators in physiology, genetics, psychology, and all those branches of research where perfect experimental methods cannot be employed, and indicates, lucidly, the assistance which statistics can render in the interpretation of results.

The Professional Training of Teachers for the Canadian Public Schools: as Typified by Ontario. By A. Gordon Melvin. Baltimore: Warwick and York, Inc. 1923. Pp. 212. Price $2.10.

The Professional Treatment of Subject-Matter. By Edgar Dunnington Randolph. Baltimore: Warwick and York, Inc. 1924. Pp. 202. Price $2.30.

These two monographs show up the contrast between the centrally organised normal school system of teacher training in Canada, especially in the province of Ontario, and the somewhat haphazard system of local government of the normal schools in the United States. The normal schools of Ontario are restricted and stereotyped by the hide-bound rigidity of the central educational authority, which prescribes absolute uniformity and immutability of methods, curricula and certifying examinations. In the United States, on the other hand, there are numerous varieties of normal schools, and each one has its governing body, its different courses, even its own theory as to the best method of producing efficient teachers. In Canada, the training given is wholly professional; while of recent years in the United States professional has been subordinated to academic training in the liberal arts. There are signs, however, that Canada, and possibly the United States also, are striving to make a nearer approach to the golden mean between these rival positions. Presumably the considerable labour which has gone to the compilation of these monographs has been undertaken in the hope of bringing about such changes.

Measuring Devices in Composition, Spelling and Arithmetic. By WILLIAM BOYD, M.A., B.Sc., D.Phil. London: George G. Harrap & Co., Ltd. 1924. Pp. 187. Price 4*s.* 6*d.* net.

This book describes how to evaluate the compositions of children of twelve years of age; and how to obtain, and test, a reasonable standard of accuracy in spelling and arithmetic at that age. It should form a practical manual for teachers, based on the use of short standardised tests and questions; whether such tests should be allowed entirely to supersede the old method of examination will appear with more certainty when they have been in use for a longer period of time. The movement in Scotland towards examination of school-children by their teachers should prove a valuable reform. With some training and experience in the use of 'fool-proof' standardised tests, the teachers can become expert examiners; and they will then be the best judges as to which children should be selected from the elementary schools to receive secondary school education.

Psychology for Students of Education. By ARTHUR I. GATES. New York: The Macmillan Company. 1923. Pp. xvi + 489.

Though written primarily for the use of students of education, this book should also prove of interest to others entering upon the study of psychology. It contains sections devoted to the physiology of the sense organs and nervous system, to general psychological principles, and to those more immediately connected with education. While the former two subjects are of necessity treated somewhat briefly and sketchily, the treatment of the latter shows well how the study of man's behaviour, especially in its dynamic aspect, can be applied to the solution of educational problems. The author successfully avoids the pose of infallibility and easy omniscence which is sometimes met with in similar publications, and this makes his book all the more readable.

The Intelligence of College Students. By ANDREW HAMILTON MACPHAIL. Baltimore: Warwick and York, Inc. 1924. Pp. viii + 176. Price $1.88.

This monograph describes at some length the selective methods which have been adopted to limit the number of those who wish to enter American Universities; and brings forward an investigation from data collected at Brown University of the possibility of a more stringent selection. The author considers that intelligence tests have more prognostic value in indicating future college success than any other educational measurement; but to avoid as far as possible any injustice they should be supplemented by a comprehensive examination, consideration of high school grade, etc. The intelligence test rank should also be taken into account in the dismissal or retention of students who have done badly at college. It is, however, the conclusion of both author and reviewer that there is no criterion which can prognosticate with certainty the academic performance of a college student.

Measuring Intelligence. By HASLAN CAMERON HINES. Cambridge, Massachusetts: George G. Harrap & Co., Ltd. 1924. Pp. xii + 146. Price 3*s*. 6*d*. net.

This book shows that not only educational science, but also the democratic ideals of the American nation may be endangered by too hasty and uncritical an interpretation of the results of intelligence measurement. Testing may be of use in the segregation of children into homogeneous classes for teaching purposes; but it should not give rise to a cast-iron grading such that individuals are forced into the vocation for which their test results appear to fit them.

How to Experiment in Education. By WILLIAM A. MCCALL, Ph.D. New York: The MacMillan Company. 1923. Pp. xiv + 281.

This book gives a detailed account of the technique of educational experimentation. The schools of the United States continue to provide a happy hunting ground for ardent experimenters on education, and Professor McCall endeavours to guide them in the selection of problems, methods and subjects, and in the statistical treatment of results.

Mental Tests and the Classroom Teacher. By VIRGIL E. DICKSON, Ph.D. George G. Harrap & Co. 1924. Pp. xv + 231. Price 6*s*. net.

While containing nothing startlingly new or original, this book should prove useful to those who wish to gain a general survey of the vogue of the intelligence test in the United States.

Improving Schools by Standardised Tests. By SAMUEL S. BROOKS, under the Editorship of B. R. BUCKINGHAM, Ph.D. Cambridge, Mass.: George G. Harrap & Co., Ltd. 1924. Pp. vii + 278. Price 6*s*. net.

A New England school inspector gives a really useful account of practical experiments in improving the educational standard of rural schools in that district. By grading the children by means of standardised and intelligence tests, the low level of attainment in certain subjects was discovered; and was raised considerably by introducing better methods of teaching and by giving to the subjects in question more space in the curriculum. Especially, oral reading was largely replaced by silent reading, which raised materially the standard not only of reading, but also of subjects such as geography, history, etc.

The Nature of Intelligence. By L. L. THURSTONE. London: Kegan Paul, Trench, Trubner & Co. 1924. Pp. xvi + 167. Price 10*s*. 6*d*. net.

A considerable portion of this book is devoted to stressing "that action originates in the actor himself." But here the author does not claim his position to be more than "after all merely a shift of emphasis" as compared with the usual view. Another extensive part of the work develops the theme that "consciousness is unfinished conduct." But here again, the author modestly disclaims originality. "To say that consciousness is unfinished action is in harmony with all schools of psychology, but the blunt statement that every mental state is an unfinished act is, at least in this phrasing, rather uncommon." Turning to his main thesis, this is that "Mental power and intelligence consist in the capacity for allowing the rough, vague, loose, almost intangible impulses to clash before they have become particularised into percepts or definitely specified ideas." "Suppose that the motive to get some money for an emergency is allowed to issue impulsively and without conscious selection at the successive stages. It may then issue as a request of a friend who happens to be near just at this moment. It might be an inopportune moment, and it might be the wrong friend....The particularisation is unintelligently said and done, and it results in failure. The above definition of the motive follows the line of least resistance without inhibition, foresight, or conscious choice of means to end." The criticism of the work would not question so much the accuracy of its statements as their relevance to the point really at issue. The need for not acting too impulsively is obvious; for otherwise the intelligence has no time in which to operate. But what we want to know is the manner in which it eventually operates. And to this question, the present volume—despite its title—does not seem even to attempt a detailed reply.

C. S.

SIXTH INTERNATIONAL CONGRESS OF PHILOSOPHY.

The American Philosophical Association, through the courtesy of Professor H. Wildon Carr, Honorary Secretary of the Fifth International Congress, appointed for London in 1915, but interrupted by the war, has been authorized by the Permanent International Committee, as constituted at the Congress of Bologna, 1911, and by the English Organization Committee of 1915, to convene the next International Congress in the United States. The American Association has also received assurances of cordial support from philosophical scholars in England, France, Germany, and Italy. It, therefore, announces its decision to invite the philosophers of all nations to meet with it in the United States in the second week of September, 1926. The place of meeting—to be more definitely announced later—will be at one of the Eastern Universities, not far from New York. English, French, German, Italian, and Spanish will be recognized as the official languages of the Congress.

In a few months the American Association intends to issue a fuller announcement of the plan of the Congress and more formal invitations to its sister associations, other learned bodies, and our colleagues throughout the world. Meanwhile, inquiries and correspondence may be addressed to the Corresponding Secretary, Professor John J. Coss, Columbia University, New York, U.S.A.

For the Organizing Committee,

Nicholas Murry Butler, *Chairman.*
A. C. Armstrong, *Honorary Secretary.*

New York, *August*, 1924.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

December 13, 1924.	A New Theory of Sensory Adaptation, by Dr C. S. MYERS.

SECTIONAL MEETINGS.

(a) AESTHETICS SECTION.

November 21, 1924.	The Relation of Aesthetics to Psychology, by EDWARD BULLOUGH.

(b) EDUCATION SECTION.

November 10, 1924.	Conscience and the Child, by Mrs C. GASQUOINE HARTLEY.
December 8, 1924.	The Nature of Intelligence and the Principles of Cognition, by Prof. C. SPEARMAN.
January 6, 1925.	The Report on Psychological Tests of Educable Capacity, by Dr P. B. BALLARD.

(c) MEDICAL SECTION.

October 22, 1924.	The Immortal Hour, by Dr H. CRICHTON MILLER.
December 17, 1924.	The Development of the Psycho-Analytical Theory of the Psychoses, by Dr JOHN RICKMAN.

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OBITUARY NOTICE

It is with very great regret that we record the death of Dr JAMES WARD, Fellow of Trinity College, Cambridge, and Professor of Mental Philosophy and Logic, which took place at his residence in Cambridge on Wednesday, March 4th, 1925. He was an original member of the British Psychological Society, took a very prominent part in founding this Journal, contributed the first paper that was published in its pages, and was, for the first three years of its existence, joint editor with the late Dr W. H. R. Rivers.

In the July issue of this Journal we hope to publish a more extended notice by Mr W. E. Johnson.

A QUANTITATIVE STUDY OF FEELING AND EMOTION IN EVERYDAY LIFE[1]

By J. C. FLÜGEL.

(*From the Psychological Laboratory, University College, London.*)

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- *Procedure* (pp. 320–321).
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II. *Feelings.*
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- *Algedonic Measures* (pp. 330–333).
- *Measures of the Variability in Feeling and their relation to the Algedonic Measures* (pp. 333–336).

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- *General Considerations on Method and Data* (pp. 337–339).
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IV. *Summary* (p. 355).

I. PROBLEM AND METHOD.

Introduction.

PHILOSOPHERS and others have for many centuries been wont to reflect upon the relative proportion of pleasure and of pain that is encountered in human life. The very divergent conclusions that have been arrived at with regard to the nature of this proportion suggest however that the methods of estimation hitherto employed have been far from satisfactory. Such methods have usually taken the form of a general review of the circumstances and conditions of life, or of judgments based upon memory as to the predominant direction of feeling in the thinker's own life or in those of his acquaintances. A very little consideration suffices to show that results achieved in this way must necessarily be extremely unreliable. With regard to the method of inference from the external conditions of human existence, it is, I venture to think, quite clearly unsafe to argue (except perhaps in the very grossest manner) from such conditions—biological, economic, or other—to the accompanying psychic states. With regard to the more direct method of reliance upon memory, it is fairly plain that even to emotionally stable individuals the value of life as measured in terms of pleasure and pain will appear to vary very considerably according to the dominant feeling tone of the passing moment; this feeling tone not only colours concomitant experience but almost inevitably biases our judgment concerning the original affective nature of remembered experiences. Changing circumstances of an environmental or physiological character also conspire to make it difficult to form an accurate estimate of pains and pleasures encountered in the past.

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[1] Based on material presented to the British Psychological Society, March 24, 1917, and the British Association for the Advancement of Science (Section J), Aug. 7, 1924. I desire to express my sincere thanks to Professor C. Spearman who read this paper in manuscript and contributed valuable suggestions. The responsibility for the methods employed and for all statements and reasonings is however my own.

The only hope of obtaining more reliable information with regard to this problem—a problem which must always be of intense interest, whatever may be our views as to the relation of pleasure and pain to the ultimate goal of human existence—would seem to lie in the resort to the more precise methods of introspection and evaluation that have been developed by experimental psychology. Reliance must no longer be placed in general impressions or vague memories covering long periods, but upon systematically recorded data obtained from adequately trained observers by the method of introspection or of retrospection applied to short and immediately preceding periods. Recent experimental work[1] has shown that the introspective method carefully applied permits of the most delicate and detailed observation of the affective processes over periods of a few seconds' duration. Such observations over extremely short periods passed under special experimental conditions are, it is evident, useless for the problem here under consideration. But the success of the introspective method as applied to the study of the feelings suggested that it might be so modified as to permit of application to this problem. It seemed not unlikely that by repeated retrospective description and evaluation of relatively long periods, during ordinary everyday occupations instead of under artificial laboratory conditions, it might be possible to collect data concerning the nature and proportion of pleasure and unpleasure in daily life, which, though necessarily far inferior in accuracy to observations made over very short periods in the laboratory, would nevertheless be much more reliable than conclusions arrived at by the more *a priori* methods hitherto employed in estimating the hedonic value of human life.

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[1] Such. for instance, as that of Wohlgemuth—"Pleasure and Unpleasure." This *Journal, Mon. Supp.* II.

Procedure.

With these considerations in mind, the present writer undertook a series of preliminary observations with a view to elaborating a method which might eventually be used by other students for the purpose of accumulating data on a sufficient scale. Observations made in this way proved to be quite possible, though very irksome. In the interests of accuracy, very frequent records were of course desirable, but it was found that the process of recording, if attempted too often, became so irritating and obsessive as to interfere seriously with the very phenomena it was desired to observe, *i.e.* the feelings as experienced in *normal* life. The most satisfactory compromise appeared to consist in making entries in the record at varying intervals, the length of which was determined largely by the nature and duration of the day's occupations. Such intervals might sometimes be as short as two or three minutes, sometimes as long as two hours. The records had two main objects: (1) a quantitative evaluation of the duration and amount of pleasure and unpleasure experienced, (2) a rough description of the chief mental states (sensations thoughts, emotions, moods, etc.) that appeared to be intimately connected with the occurrence of the various pleasures and unpleasures—a description such as might eventually also lend itself to quantitative treatment.

When, as the result of these preliminary observations, it appeared that the study of the everyday affective life by the proposed method, though difficult and troublesome, was by no means impossible, the records obtained were discussed with one or two friends, and after a few further experimental observations by the present writer and by these friends, it was decided to invite the co-operation of others. A set of fairly elaborate 'Instructions' was therefore drawn up, with a view to ensuring such uniformity of procedure as might make the results from different individuals comparable with one-another. These 'Instructions' are here reproduced almost in full; they will afford at once the most adequate description of the precise method ultimately adopted, and an indication of the suggestive influences to which the subjects who undertook the observations were exposed.

Instructions for Making the Record of Feeling.

1. The research in which you are asked to co-operate is concerned with the feelings experienced in the occupations of ordinary life. For the purpose of this research you are requested to keep a record of your feelings in everyday life, giving as full as possible an account of: (1) the pleasure and unpleasure which you experience [unpleasure is here used as the opposite of pleasure, the term pain being reserved for certain well-marked ***sensations*** (sometimes described as 'physical' pain), such as toothache, headache, the sensations from a wound, a sprain, or a burn. These in their turn should be distinguished from ***unpleasant*** sensations of sight, hearing, taste or smell, or such organic and muscular sensations as tiredness or nausea]; (2) whenever possible, the emotions or other affectively toned mental states which accompany this pleasure and unpleasure.

2. Owing to the fact that, in many cases, it is difficult to give an accurate estimate of any state of pleasure or unpleasure after this state has disappeared (cp. the very different feelings aroused by the smell of food in conditions of hunger and repletion respectively), it is desirable that the entries in the record should be made ***frequently.*** The shorter the intervals at which the entries are made, the more accurate are your observations likely to be.

The ***average*** period between each entry should not, if possible, exceed one hour.

3. You are requested to estimate your affective states in the following manner:

Suppose that 0 corresponds to a state of complete indifference, in which you feel neither pleasure nor unpleasure; that +100 corresponds to the most intense pleasure which you have ever experienced; that −100 corresponds to the most intense unpleasure which you have ever experienced. Then you will divide up this total scale of feeling (ranging from +100 to −100) into seven grades, as follows:

+3. Any state between +100 and +66.
+2. " " + 66 " +33.
+1. " " + 33 " 0.
0. A state of indifference; or one in which pleasure and unpleasure are equally balanced (or very nearly so).
−1. Any state between 0 and − 33.
−2. " " −33 " − 66.
−3. " " −66 " −100.

4. You will possibly find that, in the period covered by any particular entry, it sometimes happens that there are some contents of consciousness which are pleasant, others which are unpleasant. The figure in which you express your estimate must always refer to ***consciousness as a whole***; but, if possible, the existence of these contents of opposite feeling should be indicated. This may be done as in the following examples:

(1) Interest +, Pain − ... ... = 0
(2) Food +, Embarrassment − ... ... = −1

Here the first example would mean that you were pleasurably interested in your occupation, but that at the same time you were suffering from some pain (say a headache) which counterbalanced the pleasure, so that, on the whole, neither pleasure nor unpleasure was predominant.

The second example would mean that, while you were deriving pleasure from a meal, this pleasure was more than counterbalanced by an unpleasant state of embarrassment, caused, perhaps, by your social surroundings, so that, on the whole, unpleasure was predominant.

5. The description of the mental states accompanying your pleasures and unpleasures is left to your own judgment and discretion. Well-marked affective states such as Anger, Fear, Pain (in the sense indicated in paragraph 1), Jealousy, Hunger, Cold, are, of course, easy to recognise and to describe. Others may be more difficult to designate.

The following list of terms found in a record similar to that which you are asked to keep may perhaps be found of use, though, of course, it is by no means intended to be exhaustive:

Anger.
Fear.
Surprise.
Disappointment.
Food (the pleasures or unpleasures intimately dependent on eating and drinking).
Tiredness.
Pain.
Boredom.
Contentment.
Anxiety.
Worry.
Vague discontent.
Joy (described also as Elation).
Pleasant or unpleasant sensations not otherwise defined (recorded as pl. sens. or unpl. sens.).
Despair.
Interest (in the work or occupation of the moment).
Well-marked Organic sensations (recorded as Org. sens.).
Sexual excitement.
Reverie.
Embarrassment.
Shame.
Positive Self Feeling [cp. McDougall, *Introduction to Social Psychology*, p. 62.] (The feeling accompanying approval of Self or consciousness of one's own worth or ability. Described as P.S.F.)
Negative Self Feeling [cp. McDougall, *loc. cit.*]. (The feeling accompanying disapproval of Self or consciousness of one's own unworthiness or incapacity. Described as N.S.F.)
Positive Functional Feeling. (The agreeable feeling accompanying consciousness of the successful progress of the work, occupation or activity, upon which one is engaged. Described as P.F.F.).

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Negative Functional Feeling. (The disagreeable feeling accompanying consciousness of the unsuccessful progress or want of progress of the work, occupation, or activity, upon which one is engaged. Described as N.F.F.).

Tender emotion (described also as Love).

Sorrow.

Impatience.

Depression.

Aesthetic Experience.

The record from which these terms are taken also contains occasional references to the nature of the occupation of the moment, such as work, games (tennis, billiards, etc.), walking, cycling, theatre, piano playing, conversation. Such references may be inserted in your record at discretion.

6. The general method of keeping the record will be made clear by the following example:

8.0.	Contentment. Pl. Sens.	+1	30
8.45.	Cold. Depression ...	−1	45
9.15.	Anxiety	−1	30
10.0.	[Work]	0	45
10.10.	Anger	−2	10
10.30.	Anger (Slight irritation)	−1	20
11.15.	P.F.F.	+1	45
12.0.	„ Interest ...	+1	45
1.0.	[Work]	0	60
2.0.	P.S.F. Elation ...	+2	60
2.30.	P.F.F.	+1	30
3.0.	N.S.F. Pain (Headache)	−1	30
3.30.	Anger. Sorrow ...	−3	30
4.30.	Depression	−2	60
5.30.	Motor sensations +. Pain (Headache) ...	0	60
6.30.	[Walking.] Motor sens. +. Pain − ...	+1	60
7.30.	Elation	+1	60
8.30.	Food. Elation ...	+2	60
10.30.	Elation. Interest ...	+2	120
10.45.	Shame	−2	15
11.30.	Contentment	+1	45
11.45.	Sexual Excitement ...	+3	15
12.0.	Contentment	+1	15

In the first line of this example the figure 8.0 in the first column on the left means that the entry on this line was made at 8 o'clock. The words in the second column describe the predominant emotional and sensory content of consciousness during the period covered by the entry. The figure +1 in the third column describes the degree and nature of the feeling during the period (in terms of the scale indicated in paragraph 3); while the figure 30 in the last column indicates the duration of the period (in minutes), which, therefore, lasted from 7.30 to 8 o'clock.

The following example gives the record for a day on which the feelings experienced by the observer were much less intense:

8.15.	[Dressing, etc.] ...	0	30
9.0.	Food, etc.	+1	45
9.30.	... ...	0	30
11.30.	P.F.F., P.S.F. ...	+1	120
12.0.	P.F.F. +. Tiredness −	0	30
1.0.	... ...	0	60
1.15.	Elation	+2	15
3.0.	[Work]	0	105
3.45.	Boredom	−1	45
4.30.	„	−1	45
5.0.	Food [Tea]	+1	30
7.0.	... ...	0	120
8.0.	Boredom	−1	60
9.0.	Food. Interest ...	+1	60
10.0.	[Reading]	0	60
10.30.	Pleasant Tiredness ...	+1	30

7. If possible, the record should cover a period of not less than 30 days (as far as possible consecutive). Records covering a longer period will, however, be welcomed. The period should, whenever possible, be typical of your normal working life; holidays, travels, periods of illness or of unusual stress, excitement or monotony should, therefore, be avoided.

You are requested to start each day's record on a fresh page.

8. Absolute frankness and the greatest possible accuracy are, of course, essential for the success of the investigation. It is preferable to make no record than to make one that suffers from any considerable number of needless inaccuracies. With a view to facilitating your task, you will not be asked to sign the record. The identity of the author of any particular record should thus remain unknown to the experimenter, to whom all completed records should be sent by post. The experimenter, further, pledges himself to use the data contained in your record for strictly scientific purposes only, nor will he publish your name in connection with this investigation, except with your express permission. It is suggested that, if desired, printed characters might be used in the record, in order still further to preclude the possibility of identification.

9. You are requested to place on your record a statement of your sex and age and to answer the following questions:

(1) Do you consider that the total period covered by your record is (for you) a fairly normal one as regards the amount and relative proportion of pleasure and unpleasure?

If not, in what way is it abnormal?

(2) Do you consider that, on the whole, your life has (*a*) more pleasure, or (*b*) more unpleasure, or (*c*) about the same proportion of pleasure and unpleasure, as the average individual in this country?

(3) Are there any *special* circumstances tending to make your life happier or unhappier than the average. If so, how do they affect the period covered by your record?

You are also invited to make any further remarks which you think fit.

10. You are requested also to append to your record a list of definitions or descriptions of the principal terms occurring on your record, especially of those whose precise meaning may not be immediately obvious or whose meaning, as used by different persons, may not be quite the same. Thus you should endeavour to state as accurately as you can what you mean by such terms as Interest, Amusement, Contentment, Boredom, Worry, etc., should they occur in your record. If you do this, the scientific value of your record may be very considerably increased.

11. *In the event of your deciding to make a record, you are requested to make yourself thoroughly familiar with these instructions before you begin. This applies especially to paragraph* 3.

12. *In the event of your completing a record on the lines here suggested, the experimenter desires to thank you very heartily for your kind co-operation and for the time and trouble given to the work.*

Observers.

As already indicated, the work of making the records is apt to be extremely irksome. This is especially the case towards the end of the total period under observation; so that although a larger number of subjects attempted the task, only nine subjects completed records that covered the predetermined minimum of 30 days, and that appeared in all other respects to be satisfactory. These nine subjects had all enjoyed some degree of psychological training, in some cases a high degree. Five of them were known to me personally, while the four others were known intimately to certain friends and colleagues of mine, who were able to vouch for their aptitude and ability in psychological matters. In this connection my thanks are due especially to Dr I. B. Saxby, Major L. C. Sargent, and Mr J. W. Cox. The subjects ranged in age from 19 to 42, and included five women and four men; two of the women (nos. 5 and 8), and one of the men (no. 3) being married. In certain respects the subjects form a homogeneous and possibly unrepresentative group, since they were all working as students, or otherwise engaged to a large extent in occupations of an academic nature. They all carried out some days' preliminary observation before starting the record proper, in order that they might familiarise themselves with the method of recording.

With regard to the answers to the questions contained under paragraph 9 of the 'Instructions,' subjects 5, 7, 8 and 9 considered that the periods covered by their records, were perhaps slightly more unpleasant than were their lives as a whole. Subject 4 thought hers was slightly more pleasant—chiefly because it included part of an enjoyable holiday. On the whole, however, the subjects seem to have been, in their own estimation at any rate, fairly successful in selecting periods which were characteristic of their normal working life.

To all nine subjects my thanks are due in quite an unusual degree. Their work was in some ways far more trying than that involved in the ordinary laboratory experiment, which, however difficult and strenuous while it lasts, leaves the subject free at its conclusion to divest his mind of the somewhat obsessive worries inseparable from the introspective 'set.' In the present case, this 'set' had to be maintained throughout all the waking moments of life (no attempt was made to extend the record to dream experiences) for a whole month or more, and applied to every kind of experience, not excluding those of the most intimate or unpleasant character—a task calculated to try the patience, sincerity, and enthusiasm of even the most gifted and diligent introspectors.

II. FEELINGS.

Data.

The data obtained from the records may be convenient under two heads, according as they bear (1) upon the relati of pleasure and unpleasure experienced, (2) upon the natur proportions of the emotions and other mental states record ourselves here to (1), the first point to be considered is the the amounts of pleasure and unpleasure are to be meas present data the fundamental measure is in terms of *durati* and any answer to the question of the relative predominan or unpleasure will therefore be given primarily in terms of addition, the *intensity* of pleasure and unpleasure is record system of grading (+ 3 ... – 3). The measures for dura recorded separately for each grade of pleasure or unpleasu total of all degrees of pleasure and unpleasure, irrespective or finally some composite measure allowing both for durati sity may be adopted. We shall employ all three of these pos of measurement in one connection or another.

Reliability of Results from Individual Observers

Turning now to the actual results as measured in these important preliminary question that may legitimately be we form any estimate as to the reliability of our measur far these measures are consistent and really characteristic o life of the individuals under consideration? In this connec the statement of the observers to the effect that the period the records are fairly characteristic of their life as a whole. however, we may test the internal consistency of the rec paring one part of each record with another part of the just as two applications of a mental test to a given indivi compared in order to arrive at an estimate of the 'reliabilit For this purpose each subject's results were first calculated i for the odd days—(1, 3, 5, etc.) and the even days (2, 4, period over which the record extended. The comparison for the odd days with the totals for the even days revealed what appeared to be quite a satisfactory agreement. The features of each individual's record—as shown in the relati of the total duration falling under each of the seven gra intensity (+ 3 ... 0 ... − 3)—were for the most part quite evident in both sets of totals. This general impression was confirmed by the calculation of 'reliability coefficients,' the odd totals being correlated with the even totals for all nine individuals.

These coefficients were as follows:

Reliability of +3 values = −·03
" −3 " = +·45
" +2 " = +·96
" −2 " = +·90
" +1 " = +·90
" −1 " = +·83
" sum of all + " = +·80
" sum of all − " = +·77
" 0 " = +·80

These figures indicate that, for all except the extreme values of pleasure and unpleasure, the period during which the record was kept (a minimum of 30 days) was long enough to obtain consistent and reliable results, results which appear to reveal pretty clearly the characteristics of the different individuals who acted as observers.

The variable errors affecting our results are thus not very serious. There remains, of course, the possibility of constant errors, the most serious of which would here consist in a constant tendency for certain subjects to allot relatively high or relatively low marks for the affective intensity of their experiences, or to be biased in favour of allotting + or − values. There is unfortunately no means of estimating or measuring the extent of these errors. We may, however, fairly assume that they are smaller than those involved in the older and rougher methods of which we have spoken.

Relative Proportions of Pleasure and Unpleasure.

This being the case, we may with some confidence turn to the further examination of the records. The main problem in view in undertaking this investigation, concerned the relative proportion of pleasure and unpleasure in human life. Our records provide a quantitative answer to this problem so far as concerns that portion of the waking life of our nine observers as is covered by their reports; they further provide some data as to the proportion of pleasure and unpleasure to indifference, and the relative proportions of the different intensities of pleasure and unpleasure. The results that are of interest from this point of view are recorded in Table I. In this table the figures indicate percentages of the total time covered by the record in the case of each subject. (Thus 1·2 under heading + 3 for subject 1, indicates that the total duration of all pleasures of + 3 intensity amounted to 1·2 per cent. of the total period covered by her record, while the figure 62·2 immediately below and to the right [under the bracket] indicates that the total duration of all pleasures of whatsoever intensity amounted to 62·2 per cent. of this total period.)

The figures for the various subjects are therefore strictly comparable with one another.

TABLE I. *Relative proportion of the different degrees of feeling intensity (expressed in percentages of the total duration of the observations for each subject). [Any discrepancies are due to the fact that each value is here recorded to the first place of decimals only.]*

Subject	Sex	Age	Duration of observation (in days)	+3	+2	+1	0	−1	−2	−3
1	F	22	30	1·2	5·3	55·7	15·0	20·2	2·3	0·3
					62·2				22·8	
2	M	19	30	1·0	6·6	37·8	44·2	9·7	0·6	0
					45·4				10·3	
3	M	33	30	1·0	9·4	43·6	19·0	24·7	2·0	0·2
					54·0				26·9	
4	F	33	60	1·5	14·2	41·7	36·1	5·6	0·9	0
					57·4				6·6	
5	F	27	30	0·8	11·8	24·3	41·0	17·3	4·4	0·4
					36·9				22·1	
6	M	26	30	2·9	17·6	33·8	17·2	20·7	6·9	0·8
					54·3				28·5	
7	F	26	30	0	2·9	44·0	31·5	16·4	14·8	0·5
					46·8				21·7	
8	F	42	30	0·7	16·0	30·3	28·7	12·9	9·8	1·6
					47·0				24·3	
9	M	19	30	0·8	9·7	36·1	17·0	30·2	6·1	0·1
					46·5				36·4	
Average				1·1	10·4	38·6	27·8	17·5	4·2	0·4
					50·1				22·2	

An inspection of the table reveals several facts of interest. In the first place, the figures for every subject show a very distinct predominance of pleasure over unpleasure, so far as total duration is concerned. If we may be permitted to generalise from these results, we may therefore say that *pleasure occupies a very considerably larger proportion of human life* *than does unpleasure*. This preponderance of pleasure over unpleasure is observable not only as regards total durations of pleasure and unpleasure but also, to a large extent, as regards the separate intensities also. For all subjects the figures for + 1 exceed those for − 1; for eight out of the nine subjects those for + 2 exceed those for − 2, and for seven out of the nine those for + 3 exceed those for − 3.

In the second place the figures show that in the case of every subject, the total duration of the more intense pleasures and unpleasures is much smaller than that of the less intense feelings. This is, of course, in accordance with what a general impression of every day experience would lead us to expect. The very small percentages that occur under the heading of + 3 and − 3 are doubtless responsible for the low reliability coefficients obtained for the figures under these heads, to which we above referred.

In the third place, again in accordance with what a general impression of human life would make us anticipate, the results obtained from the various subjects differ appreciably from one another in several respects. The most important and interesting of these differences lies in the relative proportion of pleasure and unpleasure; though all subjects have a preponderance of pleasure over unpleasure, in some subjects this preponderance is much greater than in others. In one extreme instance (subject no. 4), the duration of pleasure is between eight and nine times as great as that of unpleasure; at the other extreme it is only 1·27 times as great (subject no. 9). On the average it is 2·26 times as great.

Still another interesting difference concerns what might be termed the general tendency to affectivity as revealed (*a*) in the very different percentages occurring under 0[1], (*b*) in the appreciably different proportions of the higher values of pleasure and unpleasure that are recorded by the various observers. If we neglect the relatively large middle values of + 1, 0 and − 1 and find the sum of the four remaining values for the more intense degrees of feeling, it appears that those individuals who most often experience extremes of affect (no. 6 and no. 8) have a percentage of intense feeling which is between three and four times as great as that of the individual (no. 2) who least often experiences these extremes (28·2 and 28·1 per cent. as compared with 8·2 per cent.).

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[1] It is possible however that these differences are to some extent due to individual differences in introspective ability, and in the care with which the records were compiled. Introspective studies such as those of Wohlgemuth leave little doubt that small degrees of pleasure and unpleasure are nearly always observable if the introspector is sufficiently skilled and the conditions of introspection sufficiently favourable. A relatively small difference in introspective ability between two observers might therefore result in a given experience of very low affective value being recorded as + 1 or − 1 by one observer, and as 0 by the other.

If we keep separate the sums of the high pleasure values (+ 3 and + 2) and the sums of the high unpleasure values (– 3 and – 2), we find that the individual with the highest value for intense pleasures (no. 6) has a percentage which is seven times as great as that of the individual (no. 7) with the lowest value (20·5 per cent. as compared with 2·9 per cent.); while on the side of unpleasure, the individual with the highest value (no. 8) has a percentage which is 19 times as great as that of the individual (no. 2) with the lowest value (11·4 per cent. as compared with 0·6 per cent.).

It is interesting to observe that the values for intense pleasure (the sum of the values for + 3 and + 2) correlate positively with the values for intense unpleasure (the sum of the values for – 3 and – 2) to the extent of + ·45 (P.E. ·19). This fact suggests (in view of the very high probable error it is of course insufficient to prove) that there is some general tendency for those who frequently experience high degrees of pleasure to experience frequently high degrees of unpleasure also. Such a tendency would be in harmony with the popular view often expressed or implied in the quotation of Goethe's words:

"Himmelhoch jauchzend,
Zum Tode betrübt,"

with thoughts inevitably suggested by the facts of manic-depressive insanity, with the statistical evidence brought forward by Burt[1] with regard to the existence of a factor of 'general emotionality' among normal individuals, and perhaps also with Webb's evidence[2] regarding a general factor of the 'persistence of motives' ('W').

Algedonic Measures.

Returning to the main subject of our enquiry concerning the relative proportions of pleasure and unpleasure, and keeping in view the individual differences that are manifest in this respect, it would seem very desirable to possess some single expression of the general tendency towards happiness and unhappiness that is revealed in the figures for any given subject. A convenient measure of this kind is afforded by dividing the total percentage of unpleasure by the total percentage of pleasure (*e.g.* in the case of subject 1 $\frac{22\cdot8}{62\cdot2} = \cdot37$). We thus obtain a fraction which is indicative of the general liability for pleasure to be counterbalanced by unpleasure. The higher the fraction, the greater the tendency for pleasure to be counterbalanced by unpleasure. This fraction, which I have ventured to call the 'simple algedonic ratio,' is given for each subject in the second column of Table II.

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[1] C. Burt, "General and Specific Factors underlying the Primary Emotions," *British Association Reports*, 1915.

[2] E. Webb, "Character and Intelligence" (this *Journal, Mon. Supp.* vol. I). It would seem not at all impossible that Burt and Webb's general factors are really one and the same factor studied from different points of view.

TABLE II. *Measures of the relative amounts of Pleasure and Unpleasure, i.e. 'Algedonic Measures'* (*columns* 2–8), *and of the Variability of Feeling* (*columns* 9 *and* 10).

1	2	3	4	5	6	7	8	9	10
Subject	Simple algedonic ratio	Algedonic ratio weighted according to intensity	Algedonic ratio weighted according to square of intensity	Simple algedonic difference	Algedonic difference weighted according to intensity	Algedonic difference weighted according to square of intensity	Algedonic average	M V.	σ
1	·37	·37	·36	39·4	44·2	55·6	+·44	·85	1·07
2	·23	·20	·17	35·1	43·1	61·1	+·43	·69	·88
3	·50	·45	·38	27·1	36·1	45·7	+ 36	·92	1·05
4	·12	·12	·09	50·8	64·2	102·8	+·67	·72	·87
5	·60	·54	·49	14·8	23·0	40·2	+·23	·84	1·05
6	·53	·49	·43	25·8	40·8	73·9	+·41	1·11	1·30
7	·46	·56	·73	25·1	2·3	−24·5	+·22	·79	·89
8	·52	·58	·66	22·7	27·1	34·1	+·27	1·03	1·26
9	·78	·74	·68	10·1	15·2	26·6	+·15	·67	1·17
Average	·46	·45	·44	27·9	32·9	46·2	+·35	·85	1·06

Another measure is afforded by the *difference* between the total percentage of pleasure and the total percentage of unpleasure (*e.g.* in the case of subject 1, 62·2 − 22·8 = 39·4). Here, owing to the preponderance of pleasure over unpleasure, the higher the figure the greater is the predominance of pleasure. This measure may be called the 'simple algedonic difference' and is given for each subject in the 5th column of Table II.

Both these measures are expressive merely of the relative *durations* of pleasant and unpleasant states and take no account of the *intensity* of the pleasure or unpleasure. It may, however, reasonably be contended that a measure of the relative proportion of pleasure and unpleasure in an individual's life, to be adequate, should take account of intensity as well as of duration. The above measures can be easily modified so as to meet this requirement by weighting the higher degrees of pleasure and unpleasure, before carrying out the process of division or subtraction respectively. There then arises, however, the important and difficult question as to the degree of the weighting. Are pleasures of + 2 intensity twice as desirable as those of + 1 intensity? Just how much more undesirable are pains of − 3 intensity than pains of only − 1 intensity? It is difficult to find any certain ground for deciding questions of this kind. We have therefore employed two different systems of weighting, each of which permits the calculation of a further algedonic ratio or algedonic difference. According to the first of these systems the values for + 2 and − 2 are multiplied by 2 and those for + 3 and − 3 by 3 before being included in the total + or − values respectively. We thus obtain the 'algedonic ratio (or algedonic difference) weighted according to intensity' given in the third and sixth columns of Table II. According to the second system the values for + 2 and − 2 are multiplied by 4 and those for + 3 and − 3 by 9. We thus obtain the 'algedonic ratio (or difference) weighted according to the square of the intensity' given in the fourth and seventh columns of Table II.

With seven out of our nine subjects (all except nos. 7 and 9) there is a decrease in the size of the algedonic ratio (*i.e.* an increase in the pleasure value) as we pass from the 'simple algedonic ratio' through the 'algedonic ratio weighted according to intensity' to the 'algedonic ratio weighted according to the square of the intensity.' With the two remaining subjects (nos. 7 and 8) the change is in the opposite direction. In the case of the corresponding 'algedonic differences' there is a steady increase in the size of the differences with all subjects except no. 7. Thus, on the whole, the more we weight the extreme values, the more does pleasure predominate over unpleasure. This shows that the *relative* predominance of pleasure tends in most of our subjects to be slightly greater at the more intense degrees of feeling, than at the less intense degrees; a conclusion of some interest, if it should prove to be at all generally true[1].

Another method of arriving at a single expression of the relation of pleasure to unpleasure in an individual's life, is to take the average of the percentage values as distributed between the seven different grades of intensity (see Table I). This method takes account of the 'indifferent' periods (0 in table) as well as those of pleasure and unpleasure, which alone affect the above-mentioned algedonic ratios and algedonic differences. Since the total duration of the pleasant periods is in every instance greater than the total duration of the unpleasant periods, the average will always be on the + side, and is, in point of fact, in every case somewhere between + 1 and 0. The actual values are recorded in the eighth column of Table II.

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[1] The *absolute* predominance of pleasure is, however, it should be noted, distinctly greater at the lower than at the higher intensities, as is immediately obvious from a glance at Table I.

All seven algedonic measures (three ratios, three differences and one average) correlate, as is to be expected, rather highly with one another. The actual extent of these correlations may not be without interest and is indicated in Table III.

TABLE III. *Intercorrelations of the 'Algedonic Measures' recorded in Table II.*

	Simple algedonic ratio	Algedonic ratio weighted according to intensity	Algedonic ratio weighted according to square of intensity	Simple algedonic difference	Algedonic difference weighted according to intensity	Algedonic difference weighted according to square of intensity	Algedonic average
Simple algedonic ratio	—	·81	·70	·90	·68	·51	·76
Algedonic ratio weighted according to intensity	·81	—	·97	·94	·88	·85	·92
Algedonic ratio weighted according to square of intensity	·70	·97	—	·86	·95	·90	·93
Simple algedonic difference	·90	·94	·86	—	·88	·76	·93
Algedonic difference weighted according to intensity	·68	·88	·95	·88	—	·92	·98
Algedonic difference weighted according to square of intensity	·51	·85	·90	·76	·92	—	·90
Algedonic average	·76	·92	·93	·93	·98	·90	—

Each of these methods of calculation reveals the presence of the very considerable individual differences to which we drew attention in dealing with the percentages of + and − values in themselves. These differences are more strikingly apparent however in the case of the 'ratios' and 'differences' than in the case of the 'average.' Thus as measured by the 'simple algedonic ratio' and the 'algedonic ratio weighted according to intensity' the happiest of our nine subjects (no. 4) is more than six times as happy as the unhappiest subject (no. 9 according to these methods); as measured by the 'algedonic ratio weighted according to the square of the intensity' she is more than eight times as happy as the unhappiest subject (no. 7 according to this method). These contrasts are even more marked in the case of the weighted 'algedonic differences.'

Measures of the Variability in Feeling and their relation to the Algedonic Measures.

The data we have collected and summarised in Table I afford information not only with regard to the relative proportion of pleasure and unpleasure but also with regard to the tendency to variability in feeling tone in the case of each subject. Here, as elsewhere, there is no necessary connection between central tendency and variability. Two subjects, for instance, might both conceivably have an average of + 1; but in the case of one of these the feeling tone might vary so slightly that all recorded experiences would fall without exception into the + 1 group, whereas in the case of the other the feeling tone might vary enormously from one entry to another, so that there would be a relatively large number of entries under each of the seven groups from + 3 to − 3. The percentages recorded in Table I afford some rough means of estimating this tendency to variation without further calculation. They show that there is considerable difference between the nine subjects as regards their tendency to variation, but that this difference is less striking than that as regards the relative preponderance of pleasure over unpleasure.

The most satisfactory methods of obtaining measures of the tendency to variability in feeling consist in calculating the mean variation (M.V.) and standard deviation (σ) from the 'algedonic average.' These two measures are recorded in the last two columns of Table II.

Having obtained measures both of algedonic tendency and of variability, it is natural to ask whether there is evidence of any relationship between these two. Is there, for instance, any tendency for the more happy subjects to experience greater or less fluctuation in feeling tone than the less happy subjects? Such questions should be answerable by working out correlations between the algedonic measures and the measures of variability. Unfortunately, owing to the very small number of subjects who took part in this investigation, the probable errors of the correlation coefficients obtained must necessarily be so high as to make it impossible to come to any very definite conclusion. The results are, however, I venture to think, sufficiently suggestive to be worth a brief consideration.

The following are the correlations between the measures concerned (a positive correlation indicating a positive correspondence between a *high* degree of variability and a *high* predominance of pleasure and so on):

	M.V.	σ
Simple algedonic ratio	−·18	−·73
Algedonic ratio weighted according to intensity ...	+·10	−·62
Algedonic ratio weighted according to square of intensity	+·18	−·47
Simple algedonic difference	+·03	−·53
Algedonic difference weighted according to intensity ...	+·20	−·27
Algedonic difference weighted according to square of intensity	+·11	−·34
Algedonic average	+·42	−·33

Only three of these correlations (those underlined) are greater than three times their probable error. These, it will be observed, are all negative. Further, it will be seen that the correlations with σ tend to be negative, while those with M.V. tend to be slightly positive. Now M.V. differs from σ in that it is comparatively little affected by extreme values. Extreme values, on the other hand, play a relatively important part in σ, owing to the process of squaring involved in the computation of this last measure. That this difference between the measures is really of some significance in the present case is suggested by the fact that if we correlate the subjects' σ with their tendency to experience intense feeling (as measured by the sum of the values for + 3, + 2, − 3 and − 2 in Table I) we obtain a coefficient of + ·72, whereas the corresponding correlation in the case of M.V. is only + ·50. In view then of the fact that, in the above list of correlations between measures of variability and algedonic measures, the correlations with σ are all negative and include three—the only three—that are statistically significant, there appears to be an indication of a negative correspondence between the tendency to predominance of pleasure and the tendency to frequent occurrence of the higher degrees of feeling.

This indication is supported by a consideration of certain other correlations. Thus if we correlate σ with the total percentage of unpleasure (as recorded in the figures under the brackets on the right-hand side of Table I) we obtain a coefficient of + ·83, whereas the correlation between σ and the total percentage of pleasure is + ·10.

Again, if we take the sum of the percentages for + 3 and + 2 (which we may regard as a measure of the tendency to experience 'intense pleasure') and correlate this value with our algedonic measures, we find that the resulting coefficients are not distinctly positive, as we should expect on *a priori* grounds (since the algedonic measures are of course to some extent dependent on the + 3 and + 2 values). The actual coefficients are as follows:

'Intense pleasure' and simple algedonic ratio	− ·37
,, ,, ,, algedonic ratio weighted according to intensity ...	− ·12
,, ,, ,, algedonic ratio weighted according to square of intensity	+ ·09
,, ,, ,, simple algedonic difference	− ·18
,, ,, ,, algedonic difference weighted according to intensity	+ ·16
,, ,, ,, algedonic difference weighted according to square of intensity	+ ·39
,, ,, ,, algedonic average	+ ·10

The corresponding measure for 'intense unpleasure' (the sum of the values for − 3 and − 2) gives, however, as we might expect, rather high negative correlations with the algedonic measures:

'Intense unpleasure' and	simple algedonic ratio	−·75
,, ,, ,,	algedonic ratio weighted according to intensity	−·83
,, ,, ,,	algedonic ratio weighted according to square of intensity	−·79
,, ,, ,,	simple algedonic difference	−·70
,, ,, ,,	algedonic difference weighted according to intensity	−·57
,, ,, ,,	algedonic difference weighted according to square of intensity	−·50
,, ,, ,,	algedonic average	−·59

Finally, we may note that the correlation between 'intense pleasure' and the percentage for + 1 is − ·75, whereas the correlation between 'intense unpleasure' and the percentage for − 1 is + ·37. These two last results show that what a subject gains in the way of the more intense pleasures, tends to be more than counterbalanced by a loss in the less intense pleasures of longer duration, whereas there is no corresponding process of counterbalancing on the unpleasant side.

We may thus conclude that such evidence as may be drawn from our (admittedly insufficient) data points to the existence of an inverse correspondence between the tendency to experience intense feelings and the tendency to be generally happy. If confirmed, this result is of considerable general interest for the psychology of affection, and it may be hoped that further evidence bearing on this point may soon be forthcoming, since such evidence may be obtained by methods far less laborious than those employed in the present research, which aimed primarily at determining the relative proportion of pleasure and unpleasure, not at investigating the relation between pleasure and variability of feeling. We may note however that our provisional conclusion is in conformity with the result of what is, so far as I am aware, the only other research which has produced evidence bearing directly on this question, that of Webb, who, in the course of his extensive investigation into various character qualities, found an inverse relationship between the "general tendency to be cheerful (as opposed to being depressed and low spirited)" and a "tendency to quick oscillation between cheerfulness and depression (as opposed to permanence of mood)"; the actual correlation coefficient being − ·27 (P.E. ·054), which rose to − ·38 when corrected for unreliability[1].

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[1] E. Webb, *op. cit.* p. 13 and Tables IV and VI. Webb also found a correlation of − ·79 (corrected) between "occasional liability to extreme depression" and "cheerfulness" and of + ·75 between "extreme depression" and "quick oscillation." Unfortunately he has no estimates of 'liability to extreme cheerfulness' such as are afforded by our present data.

III. EMOTIONS.

General Considerations on Method and Data.

So far we have treated the records of our nine subjects exclusively from the point of view of pleasure and unpleasure. We now turn to consider the further descriptions of the mental states recorded. This portion of our records is, in all probability, somewhat less reliable than that portion which deals with the feelings in the narrow sense, and this for two principal reasons. In the first place the main object of the present research concerned the quantitative aspects of pleasure and unpleasure, rather than the delicate qualitative analysis of pleasant and unpleasant states, and the 'Instructions' given to our subjects were therefore formed with a view primarily to the collection of data with regard to the former aspects of mental processes and only secondarily with a view to the observation of the latter aspects. The second and more important reason lies in the nature of the material itself. Both observation and description are very often easier in the case of pleasure and unpleasure than in the case of emotions, moods, etc. In the case of the former, there are only two states to identify, and there is little or no difficulty with regard to the words used to denote these states. (The word 'unpleasure' was used instead of 'pain' in order to avoid the ambiguity of the latter word.)

In the case of the other affective aspects of consciousness, there are a great variety of experiences which may have to be identified and described, experiences with regard to which there is, as yet, even among psychologists, no generally accepted system of classification or terminology. It would, of course, have been possible to adopt some definite scheme of classification and terminology for the purposes of this research. This would probably be the most satisfactory procedure in any similar investigation dealing solely or primarily with the qualitative analysis of the affective mental states. But in the present case it seemed (rightly or wrongly) undesirable to complicate the already very difficult and strenuous task which the subjects were asked to undertake, by insisting on the rigid use of certain classifications and certain terms, and on the employment of these terms in one very definite sense only. It was feared that, much superior as this latter course would undoubtedly have been from the point of view of scientific method, insistence on these further details might have led to the breakdown of the whole undertaking, through failure to enlist the co-operation of sufficient subjects. Bearing in mind the small number of records actually carried through to a satisfactory conclusion, it seems quite possible that this fear was not without foundation.

With a view however to producing some degree of uniformity in the records of different subjects, and at the same time of affording some fairly clear indication of the kind of record that was desired, there was included in the 'Instructions' a list of all the terms found in the preliminary records made by the present writer and his friends, together with two sample records showing some of these terms actually in use. There can be little doubt that—for good or for evil—this list and the accompanying records had a high suggestive influence in determining the choice of the subjects' descriptive terminology[1].

There is too, in all probability, another factor which has helped to produce a greater uniformity than would otherwise have been the case. Among the psychological treatises dealing with the affective and conative aspects of the mind, there is one which, since its first appearance several years previous to the beginning of the present work, has exercised a paramount influence on the English students of psychology; that is McDougall's *Introduction to Social Psychology*, a book which was certainly quite familiar to all my subjects, and which probably had influenced to a very large extent their thinking on those aspects of the mind with which they were here concerned. Indeed, from subsequent conversation with two of the subjects themselves and with one of the friends who was familiar with some of the other subjects who were unknown to me personally, I learnt very definitely that in several cases at any rate, the use of terms was copied from or modelled on that of McDougall. When, in addition, we bear in mind the fact that a reference to this book was actually contained in the 'Instructions' (in connection with the terms 'positive' and 'negative self-feeling'), we seem justified in supposing both that the general familiarity of McDougall's book has exercised a tendency towards uniformity in the choice of terms occurring in our records, and that as a general rule terms are used in the sense in which they are employed by McDougall.

Although no attempt was made to insist on the employment of a uniform terminology, all the later copies of the 'Instructions' that were given out contained a request to append definitions of the principal terms occurring in the record. Unfortunately only the last three (nos. 7, 8, 9) of the nine subjects appear to have worked from such copies of the 'Instructions,' but, so far as they go, the definitions showed a sufficient degree of uniformity in the use of the principal terms, a few cases of employment of a generally known term in an unusual sense being confined to mental states that were of relatively short duration or of rare occurrence.

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[1] This is pretty clearly indicated for instance by the fact that the otherwise little used terms 'positive functional feeling' and 'negative functional feeling' were very extensively employed in the subjects' records.

It will be noted that these terms (together with 'positive' and 'negative self-feeling') were—because of their general unfamiliarity and contrary to the practice in other cases—provided with a rough definition.

While freely admitting therefore that the results concerning the nature of the affective states experienced by our observers, leave much to be desired from the point of view of scientific accuracy, we may, I think, pass on to the treatment and consideration of these results with somewhat greater confidence than, owing to the rough method employed, we might at first have been inclined to entertain.

Treatment of Data.

The quantitative treatment of this portion of our data was a somewhat lengthy and laborious piece of work (there being altogether nearly 10,000 entries—many of them complex in nature—to be dealt with). Nor is it easy to devise a satisfactory method of evaluation. After a good deal of preliminary consideration and experimentation, the system described below was finally adopted. In this description (and in our subsequent considerations concerning the results themselves) the word 'emotion' will, for the sake of brevity, be used to denote any entry in a record that was definitely descriptive of an affective mental state or cognitive state closely associated with an affect; the term 'feeling value' will be used to denote the intensity of feeling recorded (on the scale $+3 \ldots -3$) for any given period; and the term 'duration value' the duration (in minutes) of a period. (Since the 'feeling values' and the 'duration values' were the only regularly recurring quantitative data provided by our records, any quantitative treatment of the 'emotions' had of course to be based upon these data.) The term 'emotion value' will be used to indicate the quantitative value ultimately allotted to an 'emotion' in accordance with the system of marking to be described.

In reading the description of the system for marking the 'emotions,' it will be well to bear in mind the general form of the records, as indicated in the examples given in the 'Instructions' (p. 4).

System of Marking 'Emotions.'

A. *Feeling Values of* +1 *or* −1.

1. When there is only one emotion entered for a period, the exact amount of the duration value is credited to this emotion. *E.g.* the entry

Contentment 60, +1

(this entry meaning that 'Contentment' was experienced for a period of 60 minutes, during which the feeling tone was of +1 intensity) would be marked

Contentment 60

(*i.e.* Contentment was credited with an emotion value of 60).

2. When two or more emotions of the same sign are entered for the same period, the duration value of the period is divided equally between them. *E.g.* the entry

$$\left.\begin{array}{l}\text{Joy}\\ \text{Pleasant Sensations} -\end{array}\right\}60,\ +1$$

would be marked

Joy 30
Pleasant Sensations 30.

(An exception to this rule was made in a few cases when the record indicated that one emotion predominated, in which case, a certain additional allowance—varying from case to case according to the nature of the indication—was made to the predominant emotion, the allowance of the other being proportionately reduced.)

3. When two emotions of opposite sign were entered for the same period, the emotion which was of the same sign as that of the feeling value of the period was credited with twice the duration-value of the period; the one of opposite sign being credited with the exact amount of the duration period. *E.g.* the entry

$$\left.\begin{array}{l}\text{Joy} +\\ \text{Unpleasant Sensations} -\end{array}\right\}60,\ +1$$

would be marked

Joy 120
Unpleasant Sensations 60.

[This was of course based on the theory—right or wrong—that the feeling value actually recorded was a result of the algebraic summation of the feelings associated with the concomitant pleasant and unpleasant emotions.]

4. When, in an entry containing emotions of opposite sign, there occurred more than one emotion of the same sign, the total value that would be allotted to a single emotion of that sign according to rule 3 was equally divided between the emotions of that sign (in accordance with the principle of Rule 2). *E.g.* the entry

$$\left.\begin{array}{l}\text{Interest} +\\ \text{Joy} +\\ \text{Pleasant Sensations} +\\ \text{Embarrassment} -\\ \text{Worry} -\end{array}\right\}60,\ +1$$

would be marked

Interest 40
Joy 40
Pleasant Sensations 40
Embarrassment 30
Worry 30

B. *Feeling values of* +3, −3, +2 *and* −2.

5. When all the emotions entered for a given period are of the same sign, the values allotted to the emotions under rule 1 or 2 (whichever may apply) are doubled in the case of periods of +2 or −2 feeling values, and trebled in the case of +3 or −3 feeling values. *E.g.* the entry

$$\left.\begin{array}{l}\text{Joy}\\ \text{Pleasant Sensations}\end{array}\right\}\ 60,\ +2$$

would be marked

Joy 60
Pleasant Sensations 60.

6. In all cases of the occurrence of emotions of opposite sign, the values allotted under rule 3 or 4 (whichever may apply) to the emotions of the same sign as that of the feeling value of the period are doubled in the case of +2 or −2 feeling values, and trebled in the case of +3 or −3 feeling values. (The values of the emotions of opposite sign to that of the feeling value of the period being the same as that allotted under rule 3 or 4.) *E.g.* the entry

$$\left.\begin{array}{l}\text{Interest } +\\ \text{Joy } +\\ \text{Pleasant Sensations } +\\ \text{Embarrassment } -\\ \text{Worry } -\end{array}\right\}\ 60,\ +2$$

would be marked

Interest 80
Joy 80
Pleasant Sensations 80
Embarrassment 30
Worry 30.

C. *Feeling Values of* 0.

7. No marks are given to a single emotion entered for a period with a feeling value of 0. *E.g.* the entry Anticipation 60, 0
would be marked Anticipation 0.

[As a matter of fact single emotions were very rarely recorded in connection with such periods, such single entries as occurred referring usually to occupations rather than to mental states.]

8. When two emotions of opposite sign are entered for a period with a feeling value of 0, each emotion was allotted a value equal to the full duration value of the period. *E.g.* the entry

$$\left.\begin{array}{l}\text{Interest } +\\ \text{Unpleasant Sensations } -\end{array}\right\}\ 60,\ 0$$

would be marked

Interest 60
Unpleasant Sensations 60.

[The assumption here being the same as that noted in connection with rule 3.]

9. When in an entry containing emotions of opposite sign, there occurred more than one emotion of the same sign, the total duration value of the period was divided equally between the emotions of similar sign. *E.g.* the entry

$$\left.\begin{array}{l}\text{Interest } +\\ \text{Pleasant Sensations } +\\ \text{Depression } -\end{array}\right\}60,\ 0$$

would be marked

Interest 30
Pleasant Sensations 30
Depression 60

Such was the system of evaluation employed in dealing with the results. Very little was attempted in the way of *classification*. The general rule has been to employ the actual categories and terms used by the subjects themselves in their records, except in a few cases, where for special reasons this seemed undesirable. The two chief instances in which this occurred concerned:

(1) *Sensations*. A very considerable number of different kinds of sensory experience are mentioned in the course of the records. To have included them all would have greatly extended the size of our lists and tables without any proportionate gain, for the emotion-values coming under each individual heading would usually have been far too small to afford any information of general interest. The different subjects varied also a good deal with regard to the degree of detail with which they analysed their sensations. I have therefore thought it best to abandon any attempt at detailed sensory analysis, and have classified all sensory experience under the two main heads of 'Pleasant' and 'Unpleasant Sensations.' The only exception to this rule concerned the pleasures of the table (classified as 'Food'), since these concern experiences recurring regularly in the lives of all our subjects, and referred to in every one of the records, so that individual differences in the hedonic value of these pleasures (as calculated from the records) may with some reason be regarded as springing to some extent from corresponding differences in mental make-up.

(2) *Fear and Anger*. A considerable number of terms are used to express different nuances and degrees of these emotions. The use of these terms obviously varied from one subject to another, and judging from the internal evidence of the records, did not always appear to be quite consistent even with the same subject. It appeared a matter of extreme difficulty to make anything approaching an accurate qualitative analysis of the entries in the records; the major quantitative distinctions, on the other hand, could find expression through the recorded feeling values.

I have therefore, here also, grouped together the various entries under two main heads, entitled 'Anxiety and Worry,' and 'Anger, Irritation and Annoyance,' respectively.

Reliability of Results from Individual Observers.

We may now, at length, pass to the consideration of our results as thus evaluated and classified. As in the case of the data concerning pleasure and unpleasure, the first question to be asked must necessarily concern the 'reliability' of our findings. What degree of consistency is shown by each subject as regards the relative part played by the different emotions he records? To answer this question, the emotion values for each subject were again, as in the case of the feeling values, at first divided into two groups, those for the odd days and those for the even days being calculated separately. Table IV gives the 'reliability coefficients' obtained by computing the correlations between the two groups for each subject, the pleasant and unpleasant emotions being here, as elsewhere, kept distinct.

TABLE IV. *Reliability coefficients of 'emotions.'*

Subject	Pleasant emotions	Unpleasant emotions
1	·79	·83
2	·85	·84
3	·99	·98
4	·90	·83
5	·93	·70
6	·94	·80
7	·88	·97
8	·35	·39
9	·90	·93

It will be seen that with eight out of the nine subjects, the reliability coefficients are quite satisfactory. In the case of subject no. 8 the period of observation appears to have been too short to obtain any very reliable information with regard to the relative importance of the different emotions. Except in the case of this subject, however, we may conclude that our results are not seriously vitiated by random errors.

Congruity of the Results from Different Observers.

Having examined the internal consistency of the records, we may next ask the question: How far do the records of the different subjects correspond with one another? To what extent do the different subjects agree as to the relative importance of the different emotions? This question can best be answered by comparing the emotion values for each emotion, as obtained from the record of each subject[1], each individual record being correlated with every other. We thus obtain the results recorded in Table V.

TABLE V. *Intercorrelations of the emotion values for different subjects.*

PLEASANT EMOTIONS.

Subject	1	2	3	4	5	6	7	8	9
1	—	·92	·71	·83	·51	·81	·18	·57	·78
2	—	—	·77	·90	·64	·88	·32	·42	·68
3	—	—	—	·69	·82	·85	·34	·67	·69
4	—	—	—	—	·52	·80	·50	·50	·76
5	—	—	—	—	—	·67	·27	·58	·57
6	—	—	—	—	—	—	·31	·67	·72
7	—	—	—	—	—	—	—	·34	·20
8	—	—	—	—	—	—	—	—	·71
9	—	—	—	—	—	—	—	—	

Av. = ·61.

All correlations of ·41 and over are >3 times the P.E.

UNPLEASANT EMOTIONS

Subject	1	2	3	4	5	6	7	8	9
1	—	·90	·66	·68	·59	·67	·49	·46	·78
2	—	—	·70	·73	·85	·78	·52	·56	·86
3	—	—	—	·88	·62	·64	·31	·31	·64
4	—	—	—	—	·71	·63	·29	·42	·68
5	—	—	—	—	—	·69	·40	·51	·70
6	—	—	—	—	—	—	·32	·45	·73
7	—	—	—	—	—	—	—	·07	·53
8	—	—	—	—	—	—	—	—	·47
9	—	—	—	—	—	—	—	—	—

Av. = ·59.

All correlations of ·36 and over are >3 times the P.E.

The fairly high positive correlations here shown indicate that there is a large amount of agreement as to the relative importance of the different emotions. Thus on the whole an emotion which plays an important part in the affective life of one subject, plays an important part also in the life of another. We must remember however that, as remarked in Section I of this paper, the subjects represent to some extent a selected population, in that they were all occupied to a considerable extent in studies of an academic nature. It is quite possible that, had our subjects been chosen from a wider field, the correspondence in their records would have been less marked. The individual correlation coefficients in Table V are of course indicative (that is in so far as they are not seriously vitiated by the unreliability of the emotion values themselves, as will be the case

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[1] As indicated below, Tables VI and VII.

TABLE VI.

Nature of experience ('Emotion')	1	2	3	4	5	6	7	8	9	10	11	12	13	14
					Subject									
	1 %	2 %	3 %	4 %	5 %	6 %	7 %	8 %	9 %	Av. % where present	Av. % (for all subjects)	% of total values	Av. rank	M.V. of av. rank
Pleasant														
Interest	13·05	15·03	16·59	15·50	12·73	1·81	35·50	29·42	3·11	15·86	15·86	15·37	2·77	1·59
Joy and Elation	4·75	4·81	17·25	·09	12·13	32·39	—	4·76	17·72	11·73	10·44	10·90	6·28	4·21
Contentment	4·03	6·73	8·30	2·82	27·90	8·28	·66	8·46	18·84	9·56	9·56	8·81	4·11	2·57
Pleasant Sensations	16·19	9·27	3·51	28·11	5·47	2·52	1·11	1·75	·68	7·62	7·62	8·74	5·33	2·30
'Positive Functional Feeling' ...	18·19	12·88	8·94	11·07	—	5·41	·94	2·28	3·64	7·94	7·04	7·09	5·17	3·26
Food	2·06	13·55	5·16	13·07	1·11	5·22	8·04	1·22	·72	5·57	5·58	5·66	5·44	2·49
Amusement	6·19	2·20	·23	5·81	—	1·53	1·12	2·28	3·62	2·87	2·58	2·88	7·89	2·72
'Positive Self Feeling'	2·06	4·56	·94	·83	2·65	6·52	·74	·25	·28	2·11	2·11	1·92	8·11	2·10
Aesthetic Experience	·64	2·27	—	6·83	—	—	·07	—	—	2·45	1·10	1·43	13·72	3·59
Tender Emotion	·36	·36	2·84	1·29	·19	—	1·64	—	—	1·11	·74	·79	11·83	3·74
Excitement	2·12	1·00	—	—	—	—	—	—	3·59	2·24	·75	·72	14·06	4·26
Sex	—	—	4·38	—	2·97	—	—	—	—	3·67	·82	·71	14·00	5·22
Rest	—	—	—	—	—	—	3·52	1·72	—	2·62	·58	·55	14·72	3·88
Anticipation	·73	3·73	—	·43	—	1·27	·28	—	—	1·29	·72	·54	13·11	3·43
Exercise	—	—	—	—	—	—	3·60	—	—	3·60	·42	·36	15·67	2·81
Recreation	—	—	—	—	—	—	3·09	—	—	3·09	·34	·30	15·89	2·41
Relief	·13	—	—	—	·19	—	·52	·78	·97	·52	·29	·29	13·50	3·45
Companionship and Society ...	—	—	—	—	—	—	·03	2·28	—	1·15	·26	·27	15·67	2·48
Surprise	·35	·53	—	·35	—	·99	—	—	—	·55	·25	·26	14·72	2·86
Hope	—	—	—	—	—	—	·49	—	·11	·30	·07	·06	16·11	1·85
Reverie	·13	—	—	·13	—	—	—	—	—	·13	·03	·04	17·00	1·00
Admiration	—	—	—	—	—	—	—	·16	—	·16	·02	·02	16·89	1·35

Table showing:

In *Columns* 1 *to* 9 percentages of total 'emotion values' as distributed among the various emotions by the nine subjects.
In *Column* 10 the average of these percentages for all those subjects who record the emotion concerned.
In *Column* 11 the average of these percentages for all nine subjects.
In *Column* 12 the percentages of the total 'emotion values' for all the subjects (differing from Column 11 in that the 'emotion values' are pooled before the percentages are calculated).
In *Column* 13 the average rank of each emotion as calculated from the ranks of the 'emotion values' in the case of each subject.
In *Column* 14 the mean variation of the averages in Column 13.

TABLE VII

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
					Subject									
Nature of experience ('Emotion')	1 %	2 %	3 %	4 %	5 %	6 %	7 %	8 %	9 %	Av. % where present	Av. % (for all subjects)	% of total values	Av. rank	M.V. of av. rank
Unpleasant														
Unpleasant Sensations	1·33	2·18	5·05	3·75	16·27	6·54	·39	10·35	7·27	5·91	5·91	5·74	3·67	3·11
Anxiety and Worry	4·59	2·73	2·63	1·99	4·44	·24	1·56	2·06	23·93	4·80	4·80	5·10	5·22	3·14
Anger, Irritation and Annoyance	4·39	7·39	3·82	1·66	5·15	5·98	4·74	5·23	7·52	5·11	5·11	4·79	3·00	1·11
Fatigue	5·34	1·80	—	—	1·11	3·15	10·76	5·58	4·03	4·54	3·54	3·37	7·22	5·23
Boredom	5·53	2·67	2·32	2·48	2·70	1·24	5·06	1·76	·76	2·73	2·73	2·57	4·67	2·52
Depression	1·53	1·33	4·22	·53	1·05	·40	2·43	·9 48	—	2·62	2·34	2·28	7·67	3·41
'Negative Functional Feeling'	2·89	1·66	4·75	·95	—	1·54	4·99	—	1·86	2·66	2·07	2·04	8·17	2·93
'Negative Self Feeling' ...	·90	·40	1·98	·48	1·48	2·92	2·15	1·52	·12	1·33	1·33	1·34	7·61	1·41
Discontent	·09	·72	1·28	·07	1·48	5·22	—	—	—	1·47	·98	1·03	12·89	5·11
Sorrow	·86	·33	2·88	·29	—	2·75	—	·92	·09	1·16	·90	·99	11·28	3·77
Disappointment	1·03	1·87	—	·13	·89	1·39	·43	2·01	1·06	1·10	·98	·77	9·94	3·05
Conflict and Indecision	—	—	—	·32	—	1·23	·11	3·05	—	1·18	·52	·59	14·78	4·54
Embarrassment	—	—	1·50	·14	—	1·20	·81	—	·08	·75	·41	·45	14·44	4·49
Impatience	·26	—	1·37	·88	—	·09	—	·03	—	·53	·29	·35	14·56	4·72
'Tired Feeling'	—	—	—	—	—	—	1·97	—	—	1·97	·22	·19	17·78	2·39
Restlessness	—	—	—	—	—	—	—	1·57	—	1·57	·17	·18	17·89	2·20
Hurry	—	—	—	—	—	—	1·60	—	—	1·60	·18	·16	17·89	2·20
'Fed up'	—	—	—	—	—	—	·87	—	—	·87	·10	·08	18·11	1·85
Disgust	·09	—	—	—	—	·26	—	·11	—	·15	·05	·06	17·39	2·15
Self-reproach	—	—	—	—	—	—	·46	—	—	·46	·05	·05	18·33	1·56
Excitement	—	—	—	—	—	—	—	·45	—	·45	·05	·05	18·33	1·56
Surprise	·19	—	—	—	—	—	·13	—	—	·16	·04	·03	17·78	1·90
Jealousy	—	—	—	—	—	—	·08	·17	—	·12	·03	·03	18·06	1·16
Distress	—	—	—	—	—	—	—	·22	—	·22	·02	·03	18·44	1·41
Pity	—	—	—	—	—	—	—	·13	—	·13	·01	·02	18·67	1·19

The values shown in this Table correspond to those of Table VI.

for those of subject no. 8) of the degree of emotional congruity between the two subjects concerned. Thus there is a very high degree of correspondence between the emotional lives of no. 1 and no. 2; they belong (if we may generalise from what are, after all, comparatively short periods of observation) to an emotionally similar type. They both differ however (in the case of pleasant emotions very markedly) from no. 7. These figures however do not, of course, enable us to say wherein such similarities or differences may consist. Before we can obtain any insight into questions of this sort we must be in possession of the full data for each subject. These are presented in Tables VI and VII.

The actual values allotted to the emotions are not given in these tables, since they are not strictly comparable from one subject to another, being affected not only by the different total durations of the periods of observation, but also by sundry factors connected with our method of evaluation. Instead of recording the actual emotion values I have, therefore, in the case of each emotion, tabulated the results in the form of percentages of the subject's total emotion values. The figures in the first nine columns represent the percentages of the total emotion values as distributed among the various emotions by the nine subjects. Thus in Table VI, column 1, the figure 13·05 per cent. under the heading of Interest means that in the case of subject 1 the sum of the emotion values allotted to Interest amounted to 13·05 per cent. of the total emotion values of that subject.

The remaining columns (10–14) summarise in various ways the results from the individual subjects. Column 10 shows the average of the percentages for each emotion as calculated from the results of those subjects who record the emotion. Column 11 shows the same average as calculated from the results of all the subjects, irrespective of whether they record the emotion or not. In order to obtain the results recorded in column 12, the total emotion values (not percentages) from all subjects were pooled and the percentages for each emotion then calculated. The results therefore represent the percentages of the total emotion values obtained from all our data. Thus the figure 15·37 under the heading of Interest means that in the pooled results from the nine subjects Interest accounted for 15·37 per cent. of the total emotion values. In order to obtain the results recorded in column 13, the emotions were first ranked in order of their emotion values for each subject. Column 13 itself gives the averages of these ranks. In certain respects these average ranks may be a safer guide to the general relative importance of the different emotions than are the average percentages of the previous column, inasmuch as they are not affected by the variation in the total emotion values from subject to subject. Column 14 gives the mean variations of the average ranks recorded in column 13.

Results Considered with reference to the Individual Observer.

The data presented in Tables VI and VII can be considered either from the point of view of a given subject (vertically) or from that of a given emotion (horizontally). Let us first adopt the former point of view, as linking on directly to the considerations aroused in connection with Table V. We must, of course, confine ourselves here to noting a very few outstanding characteristics; further details can be obtained from a study of the table.

In *Subject* 1 emotions directly or indirectly connected with action seem to be prominent. With regard to pleasant emotions, Positive Functional Feeling ranks highest, followed fairly closely by Pleasant Sensations. Excitement is unusually high, while on the negative side, Boredom and Fatigue make exceptionally high scores, these perhaps being in a measure complementary to the Positive Functional Feeling and Excitement, in the nature of a relapse after the high degree of pleasure in action.

As already remarked, the results for *Subject* 2 are very similar to those for *Subject* 1. The just-noticed tendency to the connection of emotion with action is however perhaps rather less marked with Subject 2. Thus Interest and Positive Functional Feeling have changed places, while the more passive pleasures connected with Food rank very high. There is also less Fatigue and Boredom, though there is considerable liability to Anger.

Subject 3 is remarkable on the positive side for the high place of Joy, and for the relatively large entry for Sex. This latter term seems to have been taken by this subject and by others in a rather narrow sense as being definitely connected with sexual pleasure of a sensory kind. Had it been taken in a wider sense it would probably have appeared more frequently elsewhere. Accompanying Sex, Tender Emotion has also an unusually high value with this subject. On the negative side he has unusually high scores for Sorrow and Depression and ranks highest of all subjects for Negative Functional Feeling.

Subject 4 is of interest as being easily the happiest of all the subjects. Table VI should help to inform us wherein her exceptional happiness consists. By far the largest value on the positive side is that for Pleasant

Sensations. This is perhaps to some extent due to the fact, already noted, that part of the observation period occurred during a holiday; in this holiday she seems to have indulged very freely in muscular exercise of a pleasant kind. A remark made by the subject is however worth quoting in this connection. "...Excellent health," she writes, "tends to increase the amount of pleasure I experience, and makes pain very rare, and severe pain almost unknown." It is perhaps in conformity with this and with the taking of considerable muscular exercise, that the other sensory category of Food also ranks very high. In connection with the subject's general high predominance of positive emotion values, the following remark may also be quoted: "I believe that I have also an unusual degree of the capacity for ignoring unpleasant memories or prospects and for living in the present moment or in pleasurable anticipations."

In *Subject* 5 there is perhaps a predominance of the more passive pleasures. Thus Contentment is high, while Positive Functional Feeling is not mentioned. Discontent however is high, and is perhaps in a way complementary to Contentment. From certain outside evidence that was furnished it appears that this subject has a vigorous and capable personality (Interest too, it will be observed, is high), so that a possible predominance of certain relatively passive pleasures must not be taken necessarily to imply deficiency in the conative aspect of mentality. Sex seems to be taken in the same sense as with Subject 3 (it should be noted that the subject is a young married woman). Unpleasant Sensations (connected, it would appear, with a troublesome bodily complaint) rank exceptionally high, while Anxiety and Anger (these two being to some extent dependent on certain family difficulties) are also important.

With *Subject* 6, Joy, Contentment and Positive Self-Feeling are all high. Interest, however, occupies a lower place than is usual with our subjects. On the negative side Depression is low and Worry very low, but Discontent is high, and it is possible that the term is often used by this subject to denote states of mind which would be called Depression or Worry by most other subjects.

The record of *Subject* 7 is a little unusual. It contains a greater number of terms than any other, and perhaps to some extent because of this, correlates in general somewhat less highly with the other records than these do with each other (see Table V). One or two of the terms are employed in a rather unusual sense. Thus 'Tired Feeling' is said to be 'not genuine fatigue, though favoured by this....Partly hate and discontent, accompanied by Negative Self-Feeling and Negative Functional Feeling'; for this reason, its values have not been included under Fatigue.

Tender Emotion is high with this subject, while Positive Functional Feeling and Pleasant Sensations are low. The reason for the last-named fact is perhaps connected with the employment of the important categories Recreation, Rest, and Exercise, the entries to which might possibly otherwise have largely been placed under the heading of Sensations. On the Negative side an outstanding feature is the high value of Fatigue, which is doubtless connected both with the fact that the subject was working for an approaching examination during the period of observation, and with a generally neurasthenic disposition, to which the subject confesses in her accompanying remarks. In spite of this however she is a person of unusual intellectual ability, and 'Interest' receives more marks than with any other subject.

Subject 8 also suffers considerably from Fatigue, as well as from mental conflict associated in some degree with neurosis. Depression also is unusually high, while on the positive side a characteristic feature is the category of Companionship. Pleasure from a corresponding source, when it occurs with other subjects, would appear to be classed elsewhere, especially under Interest and Amusement. The subject is a married woman and the absence of a definite sex category may be connected with the fact that her 'marriage is not a happy one.'

Subject 9 is of interest as the one whose record shows the greatest proportion of unpleasure. The chief item on the negative side is that of Anxiety and Worry which is very high. Anger, Unpleasant Sensations and Fatigue also contribute to produce a high negative total. On the positive side there is a lack of counterbalancing Pleasant Sensations, though Contentment, Joy and Amusement are high. For this subject also, the observation period was one of rather 'abnormally hard work.'

Results Considered with reference to the Emotions.

We may now, in conclusion, briefly consider Tables VI and VII from the point of view of the individual 'emotions'; for this purpose we must study the tables horizontally and refer especially to the last five columns. Columns 10–13 afford measures of the general importance of the various emotions in the whole set of nine records (the order of emotions in the table corresponds to that of the percentage in column 12). That Interest has such a large predominance among the pleasant emotions may possibly be due in part to the fact that all our subjects were to a greater or less extent engaged in study, and Interest in the sense here used seems as a rule to denote the pleasant aspect of the normal process of study, *i.e.* the positive feeling tone of that activity which constituted the regular work of our subjects. It is possible, on the other hand, that the particular occupation of our subjects has after all at most only slightly increased the values allotted to Interest. It may be that in any career an important part of the pleasures of life are directly connected with the normal exercise of the intellectual functions, with the ordinary process of attention to the task in hand or to the sights and sounds around us. If, in any future observations of this kind, the observers are selected from a wider field, it may be possible to throw further light upon this question.

There is little to astonish us in the position of Joy and Contentment. As understood by our subjects they represent the typical forms of active and passive pleasure respectively. The former tends to accompany activities, whether of mind or body. It differs from Interest in having a characteristically high feeling tone (often + 2 and sometimes + 3, whereas Interest has seldom more than + 1), and in being less restricted to, and bound up with, the process of cognition. Contentment tends to accompany the relatively passive enjoyment of a situation without desire for change or active intervention; it is closely associated with rest, repose and recovery from fatigue.

The high place occupied by Sensation, both on the positive and on the negative side, is a matter of some interest and importance. It has often been urged that the vague sensory background afforded by the coenaesthesis exercises a powerful, though for the most part indirect, influence upon our affective life. Our present results can hardly be expected either to confirm or to refute this view; for our relatively rough methods must inevitably pass by many of the finer and more subtle influences of coenaesthesis that would perhaps be revealed by more delicate laboratory observations. But our data do undoubtedly show that sensory pleasures and unpleasures of a relatively forcible and easily introspectible kind are in themselves directly responsible for a great many—in our present records nearly 15 per cent.—of our feeling values.

As already mentioned, the items classified under the head of Sensations are fairly numerous. On the positive side the sensations connected with the muscles seem to be among the most important sources of sensory pleasure. It is just possible that the fact of our subjects being engaged largely in more or less sedentary occupations may be of significance here; the pleasures of using the muscles for a walk, for instance, may be greater with one who is compelled to sit still when at work than for one whose work itself involves considerable locomotor activities; but this is no more than conjecture. On the negative side various minor aches and pains are very prominent; headache, neuralgia, etc. together with muscular and visceral discomforts being relatively frequent. All this points to the great importance of bodily health and fitness both as a source of agreeable feeling and as a means of avoiding pain. The above-quoted words of the exceptionally happy subject 4 are worth remembering in this connection.

'Positive' and 'Negative Functional Feelings' represent pleasures and unpleasures that are intimately connected with all forms of complex purposive activity; they are immediately determined by the apparent success or failure of these activities, and seem to represent the consciousness of those conditions which, according to Aristotle and many others who have followed him, are the essential determinants of pleasure and unpleasure. It is not surprising therefore that they occupy fairly important positions in the affective scale. It is probable that these feelings are considerably affected in certain abnormal (*e.g.* alcoholic) conditions and in certain forms of mental disease (mania, melancholia, etc.), though our present data afford no information on these points.

Very similar considerations apply to 'Positive' and 'Negative Self-Feeling.' On the whole, it is perhaps astonishing that the members of this latter pair do not take higher places in the affective scale than that which they actually occupy. In the case of these two, and in that of Sex, we are made to realise that the conscious affective values, as they appear in our records, do not necessarily correspond to the value of the corresponding psychic factors for purposes of conation. Indeed there is reason to believe that such correspondence is in many cases very small[1]. Some appended remarks of one of our subjects is perhaps very significant in this respect: "I find that two mental states—those connected with the ambitions, desires and estimations centred around myself and those connected with sexual desire and sexual attraction—tend so often to affect consciousness either acutely for brief periods or for longer periods as a sort of undercurrent, that I have been quite unable to do them justice in my record." We are here again brought face to face with the inevitable deficiencies of our method.

It is not surprising that 'Food' should occupy an important place.

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[1] There is therefore not necessarily any antagonism between our results and the views of such writers as Adler and Freud concerning the conative importance of self or sex respectively. With regard to Freud we must also remember that the term Sex is used by him in a very wide sense and by our observers—as already noted—in a distinctly narrow sense.

It is true that civilisation, by its insistence on dinner-table etiquette and by substituting the use of special instruments for the more natural, simple and satisfying manipulative activities (cp. the pleasure of eating a piece of pastry or a chicken bone with a knife and fork and with the hands respectively), has in some ways undoubtedly diminished the pleasures of feeding. But it has made amends for these sacrifices by the provision of ample, regular and daintily prepared meals, selected from a varying *menu* which includes delicacies from almost every part of the inhabited world, so that the process of eating may well remain one of the most constant sources of pleasure to those possessed of some degree of wealth, sound health and good digestion.

It is not always quite clear exactly what kind of experiences have been included under 'Amusement.' One of our subjects defines it as "the pleasures felt by the appreciation of a joke or funny action, word or thought," another as "the pleasures connected with experiences of an entertaining or humorous nature." It seems to be frequently associated with attendance at social gatherings of an hilarious kind, and with visits to the theatre—especially to the lighter kind of theatrical entertainment—or the cinema. Doubtless pleasure from such sources does tend to be a fairly regular feature of the affective life of many town dwellers.

On the side of unpleasure, Anxiety and Worry occupy the most important position after that of Unpleasant Sensations. Apprehension as to what is to come is the great price that man has to pay for his power of foreseeing the course of future events and adapting his conduct so as to influence them favourably. It is probable however that to a certain extent, here perhaps more than anywhere else, affect—when above a certain intensity—tends to be correlated not positively but negatively with action; there is some evidence to show that it is just in so far as, owing to external obstacles or internal inhibitions, our subjects were unable to react adequately to impending danger, that they experienced the more intense or more prolonged periods of anxiety. This, we may note in passing, is quite in harmony with the tendencies of much recent psychological theory.

Anger, in its varying degrees, is also revealed as a potent source of unpleasure; the most frequent occasions for anger are—quite in harmony with psychological doctrine—various kinds of interference with actual or desired activities. The friend who keeps one waiting for an appointment when there are important things to be done, a person who makes a request at a moment when one is preoccupied with other matters, the servant or employee who has not done the work that she was supposed to do, the collaborator who has been guilty of a foolish mistake or misunderstanding; these—to take four actually occurring examples—seem to be the kind of checks to activity that tend most frequently to engender irritation in everyday life. Underneath these relatively ephemeral disturbances it is however often possible to detect the influence—direct or indirect—of deep-seated and permanent antagonisms (frequently resembling those to which Psycho-analysis has drawn attention), which may have a profound and widespread effect on the whole affective life of the individual.

With regard to Fatigue, the comments which we have already made in connection with Sensation, are to some extent applicable. The fatigue here recorded is however primarily mental fatigue and would seem to be due either to hard study or to neurasthenia.

The figure for Boredom will perhaps be found unexpectedly high. It is probable that the minds of cultured persons accustomed to considerable intellectual exercise are particularly liable to experience boredom when circumstances are such as to present no suitable material to which the thoughts can be applied. Boredom may thus in a sense be complementary to Interest.

Depression and Sorrow may also perhaps be regarded as complementary to the active pleasurable states of Joy or Elation, while Discontent is perhaps similarly complementary to the passive pleasure of Contentment.

In the last column of Table VI is recorded the mean variation of the average rank of the emotions; a measure, that is, of the degree of variability of an emotion from one subject to another. When the M.V. is high, it indicates either that the mental state in question really plays a part of very varying importance in the lives of our different subjects, or that the term is not always employed by the subjects in the same sense. In either case we must, of course, be particularly cautious in making any inferences with regard to the general significance of this mental state for human pleasure or unpleasure. We may note however that Interest, which ranks such an easy first for emotion values shows only a small degree of individual variation. Unpleasant Sensations, however, which head the list on the negative side, have a moderately high variability (this is what we should expect if our above considerations concerning health etc. are correct). So also have Anxiety and Worry, though the M.V. for the Anger group is surprisingly small. The highest variations of all are those for Sex and Discontent.

IV. SUMMARY.

Nine subjects kept a detailed record of their affective life for 30 days or more, entering in this record the intensity of the feelings that were experienced, and the qualitative nature of the chief affective mental states.

The results show in every case a predominance of pleasure over unpleasure, the degree of this predominance varying however considerably from one subject to another.

There is some evidence to suggest that those who tend to experience most the extreme degrees of feeling are on the whole less happy than those whose feelings are usually less intense.

As a result of the observations, it has been possible to draw up a table representing the relative importance of various mental states on the affective life of our subjects. The six most important states on the side of pleasure are described as follows (the order indicating their relative importance): Interest, Joy, Contentment, Pleasant Sensations, 'Positive Functional Feeling,' Food. The corresponding states on the side of unpleasure are: Unpleasant Sensations, Anxiety and Worry, Anger (and Irritation), Fatigue, Boredom and Depression.

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THE COEXISTENCE AND LOCALIZATION OF FEELINGS

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In 1919 Dr A. Wohlgemuth published[1] a study entitled "Pleasure-Unpleasure: An Experimental Investigation on the Feeling-Elements." The experiments were done in 1915–17, in the psychological laboratory of University College, University of London, but publication was delayed by the war. At the close of the study Dr Wohlgemuth presents a list of 77 conclusions and among them are the following: (1) "*Two or more feeling-elements may co-exist in consciousness; they may be like or they may be unlike*" (W. 239); (2) "*Feeling-elements can often be localized*" (W. 242).

In 1917–18 the writer was experimenting[2] in the psychological laboratory of Cornell University upon the problem of coexisting pleasantness and unpleasantness. The conclusions of this study are just the reverse of Wohlgemuth's: (1) "Pleasantness and unpleasantness are not felt at the same time" (Y. 271); (2) "Pleasantness and unpleasantness are not localizable" (Y. 430).

The present paper is an attempt to evaluate Wohlgemuth's study[3] in the light of my own.

Reference of feeling to object. I have gone through Wohlgemuth's protocols reading with special care reports of coexisting and localized feeling. The data are much like some of my own. In every case of so-called coexisting and localized feeling one or more of the feelings is referred to an object or cause or to some other phase of experience. For example, if X represent any object, sensation, thought, attitude or other aspect of experience, the most common types of report may be represented as follows:

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[1] *British Journal of Psychology, Monograph Supplement No. VI.* This paper will be referred to as W. and page references will be given parenthetically in the body of the paper.

[2] Results are published in two papers: (1) "An Experimental Study of Mixed Feelings," (2) "The Localization of Feeling," *American Journal of Psychology,* XXIX, pp. 237–71, 420–30, 1918. These studies will be designated as Y. with page references given parenthetically in the body of the paper.

[3] Wohlgemuth's result seems to have been accepted by J. Fröbes. In his *Lehrbuch der experimentellen Psychologie,* I, pp. 191–2, 1923, Fröbes reviews my study and Wohlgemuth's, concluding "Danach verdient die Annahme der Lokalisierbarkeit und gleichzeitigen Existenz verschiedener Einzelgefühle heute den Vorzug."

Also McDougall, in order to re-enforce his belief that pleasure and 'pain' may coexist, cites Wohlgemuth's study in addition to a few lines from Tennyson. McDougall, W., *Outline of Psychology,* pp. 348–9, 1923.

$$(1)\quad X \text{ is } \left\{\begin{array}{l}\text{pleasant}\\ \text{indifferent}\\ \text{unpleasant}\end{array}\right.$$

$$(2)\quad \left.\begin{array}{l}\text{Pleasantness}\\ \text{Unpleasantness}\end{array}\right\} \text{ is } \left\{\begin{array}{l}\text{from}\\ \text{due to}\\ \text{of}\end{array}\right\} X$$

$$(3)\quad X \text{ has a } \left\{\begin{array}{l}\text{pleasant}\\ \text{unpleasant}\end{array}\right\} \text{ feeling-tone}$$

$$(4)\quad \left.\begin{array}{l}\text{visual}\\ \text{olfactory}\\ \text{gustatory}\\ \text{etc.}\end{array}\right\} \left\{\begin{array}{l}\text{pleasure}\\ \text{unpleasure}\end{array}\right.$$

$$(5)\quad \text{The feeling is } \left\{\begin{array}{l}\text{bound up with}\\ \text{connected with}\\ \text{attached to}\\ \text{residing in}\\ \text{etc.}\end{array}\right\} X$$

$$(6)\quad \text{A } \left\{\begin{array}{l}\text{pleasant}\\ \text{unpleasant}\end{array}\right\} X$$

The above list is not a complete classification of the types of verbal expression which imply objective reference of feeling[1]. The list should be compared with that in my study of mixed feeling (Y. 253–57).

Localization of feeling. I regard affective localization as simply one phase of the common tendency to refer feeling to some object. In every case of affective localization in Wohlgemuth's data the form of report is one which refers feeling to an object. The feeling is generally localized at the place of the object. "The localization of the gustatory feeling-element was generally on the tongue or in the mouth; that of the olfactory feeling-element in the nose, throat or gullet, sometimes vaguely; the feeling-element of the pain and tactual sensation was generally referred to the hand, where the stimulus was applied" (W. 204–5). When organic sensations are reflexly set up the feeling is sometimes localized organically. Also the feeling may be localized outside of the body[1] (W. 81).

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[1] It seems incredible that Wohlgemuth at the time of writing had not heard of the stimulus error in psychophysics and of a similar 'object error' in the more recent discussion of the thought processes. Wohlgemuth writes as though the discussion of instruction, stimulus error, etc. had never gone on.

See Müller, G. E., "Zur Analyse der Gedachtnistätigkeit und des Vorstellungsverlaufes," *Zeits. f. Psychol.* pp. 61–176, 1911.

Titchener, E. B., "Description *vs.* Statement of Meaning," *Amer. J. of Psychol.* XXIII, pp. 165–82, 1912.

Boring, E. G., "The Stimulus Error," *Amer. J. of Psychol.* XXXII, pp. 449–71, 1921.

The intellectual attitude. In my study of mixed feelings I noted that "Instead of reporting affective experience the subjects tend to *judge* and *decide* whether the object is or is not pleasant" (Y. 262). A judgment in the form '*A* is pleasant' should not be confused with a direct report of affective experience such as "Pleasantness was felt; this was its temporal course." The tendency to judge the object is clearly present in W.'s data[2]. In discussing the reports of coexisting feelings given by observer *X* Wohlgemuth has noted that "...co-existence is not always experienced directly as co-existence, but a judgment is arrived at inductively," and in the same connection with subject *Z*, Wohlgemuth notes that one case of coexisting feeling is a "reasoned conclusion" (W. 186, 188).

The conditions of W.'s experiment were favourable to an intellectual attitude. (1) The subjects were instructed as follows: "Concentrate your attention upon the affective side of your experience...." In three of four printed instructions for the principal experiments is added the following: "compare the feeling-tones of the two stimuli..." (W. 15). The instruction to *attend to* and *compare* feelings would set the observers in an intellectualizing attitude favouring judgment and objectification rather than direct report of feeling[1]. (2) Wohlgemuth states: "When the stimuli were given singly they were continued as long as the subject wished; when they were given in pairs, they were of equal duration..., but the pairs were repeated on request of the subject" (W. 13). This method implies that the chief task of the subject is to pass judgment upon the stimulus-object in terms of pleasantness and unpleasantness.

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[1] In my former study I listed four types of affective localization (Y. 426–27) but I did not note any case of localization outside the body. In a more recent experiment I have found this form with one subject.

W.'s data contain a good many reports which are ambiguous regarding the place of affective localization. The bare statement that feeling was localized tells nothing of the place.

Affective localization is more frequent in W.'s data than in my own. It occurs in 34 per cent. of W.'s reports (computed from table on W. 204) and in 3·2 per cent. of mine (Y. 422).

[2] See, for example, Subject *X*, report 41. "First sensation on the whole agreeable, although I could see it was a colour I had expressed a dislike to. The second was uncertain. In attempting to judge whilst it was exposed there was a fluctuation, the decision being now that it was pleasant and then that it was unpleasant....No clear judgment emerged in the second case, whilst the first was on the whole pleasant."

Subject *X*, part of report 109: "A part of the difficulty of comparing the feelings simultaneously arises from the difficulty of appreciating the true character of the different sensations each for itself, thus as to the olfactory sensation in this experiment it was some time before I could make up my mind as to what it was (*i.e.* the real experience itself) and secondly whether it was or was not agreeable."

Subject *X*, part of report 142: "I seemed at the same time to know that there was a certain quality, a sort of whistling quality, connected with the tone that was disagreeable without feeling it to be so."

Subject *Z*, part of report 132: "I was not conscious of any alternation between the two feeling-tones for the most part, but on the other hand I was not conscious of the feeling-tones co-existing, but I conclude that they did from the stated fact of absence of alternation."

The intellectual attitude gives rise to judgments which attach the *meaning of pleasantness* or the *meaning of unpleasantness* to an object (Y. 427, 428). Reports conditioned by the cognitive attitude doubtless imply affective experience in many cases, but they are not direct reports of feeling and are ambiguous as to what was felt, how long it was felt, when it was felt, and other points of first importance to the affective psychologist. These matters must assuredly be taken into account in any experiment upon the coexistence of feelings. If a subject reports 'the object was *P*' we, of course, cannot doubt that the *meaning of pleasantness* attaches to the object but we have no guarantee that pleasantness was *felt*. We may consider it probable that pleasantness was felt, but we never arise from probability to the relative certainty of direct report[2] (Y. 258).

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[1] The instruction 'attend to' actually gave rise to a good deal of confusion (W. 16–17) and so special experiments upon the relation of feeling and attention were undertaken. The results of these experiments need not detain us (W. 222–27).

[2] The problem of the relation between meaning and affective process is of first importance in modern affective psychology. See, for example, a recent study: Yokoyama, M., "The Nature of the Affective Judgment in the Method of Paired Comparisons," *Amer. J. of Psychol.* XXXII, pp. 357–70, 1921.

There can be no doubt that some experiences *mean* pleasantness in one aspect and unpleasantness in another. Experiences of this sort are uncritically taken to be mixed feelings.

I recently asked an elementary class in psychology the following question: Have you ever experienced a mixed feeling? *i.e.* a feeling in which pleasantness and unpleasantness were together in experience at the same moment? There were 148 positive answers; 85 negative answers; 6 doubtful and ambiguous answers. Illustrations of the so-called mixed feelings are: eating candy with a tooth ache; dancing with sore feet; falling into water on a hot day; listening to music when desiring to sleep; eating pie which is too hot; seeing an old building burn; recovering from illness; graduating from school. Such comments are almost worthless scientifically; they show only that popular opinion is divided upon the question of mixed feeling.

Interpretation of mixed feelings. A study of Wohlgemuth's data confirms the view that mixed feelings depend upon an attitude of observation which may be characterized by (1) reference of feeling to object, (2) a tendency to localize feeling at the place of the object, (3) a tendency to pass judgment upon the object in terms of pleasantness and unpleasantness.

That so-called mixed feelings depend upon an attitude of observation is shown by the following evidence which is presented in summary form.

(1) There is a well-marked tendency for mixed feelings to be reported in groups of successive reports. If the reader will plot by number the introspections in which coexisting feelings are reported (for data see W. 161–63) the grouping will be clear. In my own experiment (Y. 246–48) it was discovered that mixed feelings are reported in sporadic groups throughout the course of the experiment and this was taken to mean that mixed feelings depend upon an attitude of observation which is assumed at certain times by some of the observers.

The above statements are true also for localized feelings (Y. 423–24).

(2) The data show that there is a statistical association between coexisting and localized feelings. When a subject reports coexisting feelings he is apt also to report localized feelings. This implies that one and the same attitude of observation is involved in both cases.

(3) W.'s subjects report with equal facility[1] coexisting feelings of the same quality[2], *PP* or *UU*, and those of opposite quality, *PU*. Also there are cases of three coexisting feelings of the same or different quality, *PPU* or *PPP* or *UUU* (W. 186). I have even found one case of *UI* in which the "unpleasant coldness of no. 4 at first co-existed with the indifference of no. 1" (W. 36).

In my own experiment there were a few reports of 'co-existing feelings' of the same quality (Y. 251–52) and these were called 'multiple feelings.' It now appears that the distinction between 'mixed feelings' and 'multiple feelings' is of very little importance since both types involve the same sort of observational attitude[3].

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[1] Wohlgemuth concludes, however, that "*There is some indication that it may be easier for two pleasant feeling-elements to co-exist than for two unpleasant ones*" (W. 240).

[2] I am using the following abbreviations: P=pleasantness; U=unpleasantness; I=indifference.

Unfortunately Wohlgemuth has used P as an abbreviation for pain. Also it is unfortunate that Wohlgemuth has used the rather gross term 'pleasure' and the unusual form 'unpleasure.'

[3] The fact that the subjects report two or more coexisting feelings of the same sign might be taken as an indication that there are different qualities of P and of U; but Wohlgemuth concludes: "*There are only two qualities of feeling-element, viz. Pleasure and Unpleasure. Any differences except intensity, duration and extensity, are apparent only, and are found to belong to sensations, or other cognitive or to conative processes*" (W. 235).

(4) Wohlgemuth concludes that the ability to apprehend co-existing feelings (W. 240) and the ability to localize feelings (W. 242) improves with practice. What W. regards as improvement with practice I have regarded as habituation to an attitude of observation (Y. 253–57, 265).

(5) Wohlgemuth concludes that there are great individual differences in the apprehension of coexisting feelings (W. 240) and in the ability to localize feelings (W. 243). These conclusions verify my own (Y. 248, 424).

In speaking of individual differences it should be remembered that Orth, Hayes and Alechsieff all concluded that mixed feelings do not exist; and that Johnson, Nakashima, Koch, Kellogg and Wohlgemuth have concluded that mixed feelings do exist (Y. 238–40). Five of my nine subjects reported no unequivocal mixed feelings while one subject reported more than half of the total number (Y. 248).

These gross differences have been explained by me through a demonstration that wherever mixed feelings are reported a certain objectifying and intellectualizing attitude of observation is involved. Wohlgemuth, curiously enough, has noted that: "*The ability to localize a feeling-element depends to some extent upon the attitude of the observer towards the feeling-element. The more his attitude allows him to objectify the feeling-element the easier it is to localize the feeling-element, or possibly vice versa*" (W. 207, 242).

When the subject's report is directly descriptive of feeling and unambiguous regarding temporal relations, coexisting feelings are notoriously lacking. I do not know of a single case in the experimental literature, and my own experiments upon the coexistence of *PU* alone contain more than 2000 reports.

Criticism of Wohlgemuth's experimental method and logic. The programme of Wohlgemuth's experimental study is far too general as will appear from the following quotation: "I come now to the purpose of the investigation recorded in these pages. A great many theories as to the nature of feeling-elements have been advanced, yet the greatest possible divergence of opinion still obtains concerning the most simple properties of this class of conscious processes. To elucidate, then, by carefully conducted experiments, and, if possible, to settle some of the most hotly controverted questions in the psychology of feeling were the ends for which this investigation was undertaken. I also kept in view the possibility of ascertaining why it has happened that some of the most careful experimenters and observers have arrived at absolutely contradictory results" (W. 4–5). There is no single problem, but many: the question of the relation of feeling and attention; the problems of memory and feeling; the question of the coexistence of feelings; the problem of mood, and others as well (W. 14–20). Surely in a field of psychology where there are many 'hotly controverted questions' the safest policy is to select one problem and to carry that through, if possible, to a precise and final conclusion. The over-generality of the study is again reflected in the 77 conclusions which are drawn relative to feeling (W. 233–49).

A further criticism goes to the heart of the introspective method. Wohlgemuth writes: "To give extracts only, or to state that such an observation occurred so and so many times is altogether unsatisfactory as it conveys only the experimenter's conclusions, and will seldom convince an investigator who has previously arrived at different conclusions" (W. 11). Although Wohlgemuth prints complete protocols he resorts to the very method which is condemned when it comes to drawing his own conclusions. From what is known regarding the variability of observation and of testimony it is clear that a convenient selection of cases may 'establish' contradictory propositions. Wohlgemuth's method resembles that of an investigator who collects opinions upon some matter of dispute and on the basis of these opinions concludes 'this is so' or 'that is so.' Such a method can lead only to contradiction and confusion. Had Wohlgemuth concluded merely that the subjects report coexisting feelings, I would agree immediately. When, however, these reports are generalized into a universal proposition I demur.

The true problem begins where Wohlgemuth ends. How can we account for the fact that some observers report 'coexisting' and localized feelings while others (those of Orth, Alechsieff, Hayes and five of my eight) do not?

Conclusion. At present there is no acceptable evidence that affective processes coexist. An analysis of Wohlgemuth's study shows that so-called coexisting feelings depend upon an attitude of observation which may be characterized by (1) reference of feeling to object, (2) a tendency to localize feeling at the place of the object, (3) a tendency to judge the object and to compare objects in terms of pleasantness and unpleasantness. This attitude of observation gives rise to a type of report which is ambiguous as to what was felt and as to the temporal relations of affective experience. It also favours a confusion between *statement of meaning* (of pleasantness or unpleasantness) and *direct report* of affective experience.

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PRIVILEGES AND LIMITATIONS OF VISUAL IMAGERY[1]

By T. H. PEAR.

(*From the Department of Psychology, University of Manchester.*)

1. *Introductory* (pp. 363–365).
2. *Are we caged in our imagery?* (pp. 365–369).
3. *Privileges of visual imagery* (pp. 369–371).
4. *The 'shaking-down' of an experience* (pp. 371–372).
5. *Is the verbaliser caged?* (p. 372).
6. *The possibility and importance of a visual logic* (pp. 372–373).

1. *Introductory.*

THERE may be psychologists who will regard the whole of this paper as a pitiable regression, clearly traceable to an infantile fixation upon certain aspects of the mother-science. For this lapse, I gather from the literature, there are several remedies, not clearly separable from each other. One is that anybody tempted to write thus should use his faculty of psychical synthesis. Another requires him to pull himself together. A third suggests that he might profitably appeal to the self-regarding sentiment; a fourth, that to these troublesome and chronic problems of imagery he should murmur—preferably while the subconsciousness outcrops, and bearing in mind the tricky quantitative relations between imagination and will—"Ca passe." A fifth, not clearly distinguishable from the fourth, is that he should resolutely talk of the fiction of imagery, and assert in print that he can get on without it; a sixth that he should practice auto-ergotherapy, should get down to some really useful work, on intelligence tests, motion-study or mental healing, and arise months later, with these puling doubts properly assimilated in his personality.

Now with the utmost deference to all these doubt-removing agencies, I would like to suggest that regressions to earlier forms of behaviour are not wholly deplorable. The danger of growing up too fast sometimes threatens psychology. Occasional 'retreats' are at least as desirable for the psychologist as for the saint. They may shift the relative positions of foreground and background in his mental scenery, perhaps temporarily, perhaps for ever.

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[1] Paper read at the Toronto meeting of the British Association for the Advancement of Science, August 7th, 1924.

But the point of view in this paper may represent not a regression at all, but an overhauling of mechanisms while the engine is still running. The view which this article will attempt to express is that, towards the important problem of the extent to which different kinds of imagery affect the mental life, most of us are Laodiceans or behaviourists. Mental images exist; that is a naked fact; that they can be contributed by different senses is another; that in different persons the predominant imagery is from different senses, a third. Yet I cannot help feeling that if this be true, our lowliest dealings with any parts of reality, other than those which we perceive, must be seriously affected by the type of imagery which, usually without asking leave, appears from nowhere and obtrudes itself before our inward eye, ear, nose or whatever it may be. This is what in our more arrogant moods we call 'employing' imagery. (But up a short side-path here lies the question of Free-will.)

Now these warmer and more intimate visits of imagery which occur when we simply remember things (if simple remembering ever happens) do not seem recently to have formed the favourite subject-matter of many psychologists. Rather they appear to have 'distanced' at least their style if not their material; writing abstractly about the process of abstraction, generalising about generalisation, and, naturally enough, reasoning about reasoning. Seldom do they refer to the humbler process of imaging for imaging's sake. Even then, it is often in the manner of an art critic making his one inevitable and expected reference to the pictures seen in Christmas Numbers, and thereby increasing the suspicion that anyone who really knew why the majority of people like such pictures would have something valuable for a theory of aesthetics. But let us suppose that some day these writers on the meaning of images may be persuaded to observe their material through a convex rather than a concave lens, the following custom of investigators in other branches of biology. It might then be a little easier for persons who habitually think with images to understand, or even to continue to read these publications. At present, such readers feel uncomfortably that these articles are supposed to refer to the processes of thinking with images. But introspection, unfortunately, merely increases their awareness of the extent of individual differences.

To answer, if not completely, a question in advance. "So far as I understand these carpings," a critic may say, "I gather that you think too many writers have written generally about the general functions of imagery in general, and that you would like to read more about the concrete functions of this or that concrete image, or at least of this or that type of imagery. This being so, are not Abana and Pharpar...; have you not what you want in the researches of the laboratory?" If such a question be asked, the answer should, I submit, be plain to anyone who after a week's work in any laboratory, psychological or other, has sat down calmly and asked himself what he has been driving at. The laboratory is a place in which to propound problems; investigate many, solve some, and indicate the possible ways of finding the answer to others—outside the laboratory walls. In psychology, the laboratory is a valuable setting and not a substitute for introspection. And the experiments upon memory and thinking shew clearly that though many acts of introspection could be carried out quite well if their contents chose to appear when one was in a laboratory, yet the casual habit of many interesting materials is to turn up in consciousness at other times; during apparently unprofitable reverie; when one is making real practical judgments, with serious consequences, and most of all, perhaps, when one is trying to express oneself in speech or writing. In this connexion Mr St John Ervine's remarks upon the difference between the spoken and the written style are full of psychological interest. Anyone who will attempt, in one and the same day, to write an article, to lecture to a visible class, and then from the padded stillness of a broadcasting studio to address an unseen, strange, and for all one knows departed audience, will collect material for much serious psychological thought.

2. *Are we caged in our imagery?*

My own interest in the part played in the life of different individuals by imagery-differences began when I met with Dr John B. Watson's treatment of the matter in his *Behavior*[1]. After a symposium upon this book one speaker remarked "how boxed up we are in our own particular imagery!"

Such a view led me to regard a predominant type as a kind of cage, restricting one's freedom of movement to a part of the whole world of potential knowledge. This impression was deepened when I inquired of others, not *what* they thought or remembered, but *how*, *i.e.* in what terms and with what images or other supports, they performed these acts.

Dr Henry Head has told me that during his co-operation with Dr W. H. R. Rivers in their investigations upon the nervous system, their mental differences were strikingly exhibited. When Head once compared a cutaneous sensation to a visual sensation, "as a light in the darkness," Rivers laid down his pen, saying, "I cannot write that down as a serious contribution to science." Yet that is the aptest way in which the appearance of the cutaneous sensation could be expressed by a strong visualiser.

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[1] Henry Holt, New York.

Until recently, I had accepted rather compliantly this idea of the box, cage or rabbit-run as illustrating the power of imagery to limit one's appreciation of the number of things of which this world is so full. Yet perhaps a psychologist may exercise for a moment his privilege of becoming autobiographical. During the last few years my kinaesthetic imagery—or kinaesthetic sensibility, for perhaps kinaesthesis has no imagery, and I concede this merely as an insurance against the danger of being side-tracked at this point—has become more differentiated and possibly enriched. This I take to be a result of increased theoretical and practical interest in muscular skill. In consequence of this I now find myself more able to anticipate certain experiences in kinaesthetic terms. (In the last sentence I almost wrote *envisage*; thus betraying my bondage.) Moreover, it is personally gratifying to notice that the muscle-groups concerned in such adventures are the larger ones; those of the arms, legs and trunk, and not those tiny, finicking overworked teams which produce speech or writing.

By analogy with the 'motile' whose visual imagery, though poor and sparse, occasionally brightens up sufficiently to give him a glimpse of the world of pictorial memory, I venture to suppose that I am beginning to pick up crumbs from the rich man's table. These transient glories of kinaesthesis thrown towards me are perhaps the staple diet of some big hulking motiles. They, maybe, are *blasé* towards them, as I am towards visual imagery. Yet for me, such new experiences are the voyages of a Columbus.

All this refers only to my remembering of events actually experienced; to reproductive memory. Yet there are occasional moments in which such kinaesthetic experience performs a loftier function. It acts as the vehicle for a meaning; as a material foundation for a general thought. Presumably kinaesthesis does this sort of service almost continually for some people, but an example from my own experience may make this clearer.

I was acting as subject in some experiments upon musical appreciation. My almost exclusively visual interpretation of the piece played on the gramophone (Tchaikowsky's "Miniature Overture" Casse-Noisette Suite), was suddenly interrupted by a kinaesthetic experience of myself skating a 'forward spiral.' The muscular tension (which is considerable) involved in throwing the whole body forward, was very noticeable. The duration of the spiral coincided exactly with that of a passage in the music, and the pleasure caused by it was intense.

I suspect that the whole muscular performance was very much better in imagination than it would have been in reality. But the interesting thing to me, as a visualiser, was that the feeling of power and freedom given by the body's swoop along the spiral curve *represented* the music. It did so as adequately, as a few bars before, the (to me) more commonplace visual image had done.

Here comes a problem. In such an experience, had I escaped from one cage merely to visit another? Or is it that images may be less like cages than search-lights, illuminating the facts of existence in their own unique and inimitable way? May they be like *coloured* search-lights, rendering visible certain parts of the field, and inevitably throwing others into shadow? May the illuminations and obscurations be peculiar to the colour, so that a ray of another hue would illuminate quite different patches from the same area?

The result of these goings-on in the physical world is notoriously startling. By such means the costumes of actors on a stage can be made to appear entirely different, and the scene may be completely changed. But I suspect strongly that something similar to this characterises the world of mind. A visile[1] gets through life so successfully in many ways that he may never pause to consider the advantages or the disadvantages of his own or of the other types of imagery. It is just as unlikely that he will hit upon a more interesting discovery; that he and the others live in different memory-worlds. A cursory glance at Helen Keller's writings should make this clear.

The memory-world and the thinking-world of a person who never uses visual imagery would seem to me comparable to the perceptual world of an animal without eyes. The peculiarity of a visile with few motile tendencies is that he resembles an organism anchored to one spot, but privileged to watch a moving panorama much more varied than prisoners usually see. Yet there is another puzzle which my own experiences emphasise. My world of memory is nearly always silent. Yet in actual life my hearing is good. I am fond of listening to music and am rather acutely sensitive to the characteristic sounds of certain objects while perceiving them. For instance, I never get quite used to certain pleasant and unpleasant accents and dialects. In perceptual experience I seldom overlook them. In memory, however, when a reference to these aspects occurs, it is seldom carried by auditory images. I may *know*, for example, that I have heard a voice which is soft, gentle and low. This knowledge undoubtedly influences my general impression of the possessor. Yet I suspect that if the pleasant or unpleasant voice presented itself as an auditory *image* it would skew my opinion much more powerfully. My reasons for this belief are two; the effect when such auditory images do occur, and my constant experience of the dominating —or domineering—visual image.

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[1] I employ this and the homologous terms audile and motile to designate people whose predominant concrete imagery used in reality-thinking is supplied by sight, sound and kinaesthesis respectively. The person whose thinking is chiefly done with words will be called a verbaliser whatever the form in which his words may appear in consciousness.

In passing, notice might be taken of the fact, well known to psychologists, that some persons seem to be more conscious than others of their confinement in imagery, if confinement it be; while others are like that pathetic tiger which, after escaping from the menagerie, was found sneaking back to its cage, apparently unable to face the menaces of its new world. Perhaps—who knows?—the visile distrusts words and the verbaliser shuns visual images for some similar reason.

Another point, sometimes slurred over, is that even a visile whose characteristic imagery dominates his thinking may never have explored the whole of his cage. Like certain types of Englishmen, he may use only a part of his house for residential purposes. I wonder how many visiles image movement or colour adequately. For me, to visualise the movements of a person seems to be becoming easier since I became technically interested in these matters. But even so, many of my images are really more like an artist's depiction of movement by static paints and canvas, than the cinematographer's representation of it. I am inclined, too, to doubt my power of visualising movements which I have not studied attentively. Perhaps this lack of development may be bound up with the fact that my visual images are usually fleeting and impermanent unless voluntarily held[1].

The same fact may account for the uncoloured nature of my visual images, if no express attempt be made to see them as coloured. Even then, the result is dim. However, I have lived for years in a place where the sun seldom gets a chance, so this achromatism may be an accidental feature. In any case the image usually loses its colour quite soon.

These defects, even of well-marked imagery, are further evidence of the limiting or caging power of such mental contents.

My images are usually flitting grayish sketches. They carry the essence of many situations as compactly and adequately as cartoons. To such grander achievements they presumably stand in the relation of unpretentious first cousins, or as the country mouse to the town mouse. Yet, I should think, most of these sketches are really caricatures; if I am right in assuming that a caricature need not necessarily *ridicule*. The first dictionary to hand obligingly supports this supposition by giving the meaning of 'caricature' as "to draw an exaggerated or grotesque resemblance of."

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[1] Professor E. B. Titchener's and Miss C. W. Perky's suggested distinction between images of memory, which fidget, and images of imagination, which may stay still, would be relevant here if I could be convinced of its validity. *Textbook of Psychology*, and *Amer. Jour. of Psych.* XXI, 1910, 422–52.

'Exaggerated resemblance' is a seductive phrase beckoning one into dark channels leading to logic and metaphysics. Many psychological explorers have sailed along them, but with unstopped ears. So they have been lost to psychology for ever; and this is one reason why we know so little about imagery. But if a resemblance *can* be exaggerated, these sketches do it. They set a standard higher than that of the original. The original is thereby occasionally encouraged to catch up.

Perhaps this exaggeration in the image is moulding it nearer to the heart's desire. But here dashes up the eager psycho-analyst; and, he, qua psycho-analyst, must be kept out of this paper at all costs in order that the discussion may centre around, or even be about imagery.

That meanings so general as faithfulness or common-sense are apt in the visile to be carried by sketches bearing 'exaggerated resemblances' to persons who exemplify these qualities is a commonplace to some psychologists. Yet one should never forget it when reading their works. There are times when I find it very difficult to dissociate my memory of what Jung actually wrote when he attempted to distinguish introvert and extrovert, from insistent images of *Punch* cartoons of two famous Prime Ministers. It was Mr *Punch*, not Dr Jung, who quoted a description of one of them as "that island surrounded by urbanity." But the phrase seems to be slipping over from the part of my memory concerned with London to the smaller area occupied by Zürich. This overlaying of the less by the greater is a sign of weakness, a verbaliser might say, and this time his strictures seem justified.

3. *Privileges of visual imagery.*

Now this fact, that the general meaning of a concept is often carried in the visile's mind, not by a photographically accurate image—if such things ever exist—but by a caricature, seems to have been neglected by persons who regard visual imagery as characterising minds of lowly organisation. Visual imagery having had a longer history than verbal imagery, may have developed powers of expressing discrimination, abstraction and generalisation to an extent not yet adequately realised.

Almost certainly, there is a technique of arriving at truth along visual routes. It is worthy of description for its own sake. It may even be developing now. This is quite apart from the possibility that as a means of arriving at truth it is riskier and less communicable to others than the verbal method. The quickest way from the sea-shore to the top of a cliff may be by aeroplane. It is comparatively safe, it is direct, and its conveniences are public property, or, at least, available to all who can afford them.

The practice of thinking in words seems to be like this, and it, too, must be paid for, in fees to schools, colleges, tutors, bookshops and the like. Yet some people mount cliffs by shinning up their faces. It may be, and often is, a primitive way. But we do not confuse the climbing of the small child with that of a member of the Alpine Club. Cliff climbing has a technique, a literature, perhaps even a poetry which is apparently not easy to communicate to others.

Now, can we say that the visile attacks reality by climbing up its face, while the verbaliser sails up parallel to it, maybe in the armchair of a public aeroplane? I believe that occasionally this would be a fair comparison. That there are many visualisers who choose easy, well-trodden rock-faces, and plenty of stern verbalisers who experiment eternally with their own private aeroplanes, battling with unknown and unexpected cross-currents and pockets in the misty atmosphere of thought makes it clear that this is by no means a moral distinction. But the meaning of the words to such verbalisers must then be private property. And when this is so the experience of the thinker new to such flights must be like that described in Mr C. E. Montague's *A Hind Let Loose.*

> To fit word on thought—no; to say this made two things of them—but to hold the thought, to force it up and up the scale of clarity to where it and some unsought word rushed together and a new thing came to life, as when Adam first spoke—to Dick this was the great, the one game; the gift of speech might have only come in last night; there he was, at his first day's fun with it, wild in love with all that the living, limitless thing might work out to.

With all diffidence however, it may be suggested that in one respect visual images are superior to words. We exchange words with each other. As a result, though the psychological meaning of words is often intensely personal, many meanings must inevitably become public property. This brings in its train the drawback that words convey other people's meanings to you as well as your own, even at times when you might wish them to be personal. This is painfully obvious when, trying to write an intimate letter of condolence to a close friend, you feel that all the words you wish to use have had their edges worn off.

Perhaps, in some way the poet can demagnetise these words of their municipalised connotation, giving them a fresh start in the reader's consciousness. The visual thinker, on the other hand, possesses private, intimately subjective pictures, and almost always, however communicative he may be, he keeps them to himself. For since people commonly exchange sentences rather than sketches of their inner consciousness, the visile is seldom called upon to stand and deliver his meanings except through those official media called words. But even if one knows the words of a visile, one may have but a slight acquaintance with the meanings which they inadequately represent. When, stranded in a foreign town, you consult the English consul, it would be naïve to assume that he is typical of England, or even that he is an Englishman. Sometimes his nationality has become sadly battered as a result of always representing and never *being*. So it is with some words; they began well, but have worn badly. The career of the word 'priceless' is a tragedy.

Visual images (and perhaps auditory ones too), possessing their own personal appropriateness, may be superior to words as houses and gardens are superior to hotels and public parks. This analogy works both ways. Their disadvantages are obvious.

Visual images, too, get their edges worn off occasionally. But their attrition is less because they are not shared with other people, while words are. And it is easier and more natural to get a new visual image, when the old one becomes inadequate, than to find or invent a new word.

4. *The 'shaking-down' of an experience.*

It is well known that any experience when recalled to memory is usually revived only in part. Moreover, some of its aspects are completely omitted. In consequence of this alone, other aspects acquire undue and unfair prominence, even if they have not been reinforced by the process of condensation with material from other similar memories. Now, whenever this omission and undue favouring are due to peculiarities in the rememberer's predominant imagery, this fact should be important for any theory of remembering or of thinking. One person's approval or disapproval of another may rest entirely upon, say, a visual basis. These aspects of the person (or more strictly speaking, the partial aspects) which form the criteria of judgment may perhaps not be *propria* but accidents, if we may borrow the language of logic.

This might involve our asking "What *are* propria in the world of visual memory?" To this question, in so far as it relates to human beings, Professor Knight Dunlap has offered a partial answer in his *Personal Beauty and Racial Betterment*. The *propria* may be biologically important, and almost certainly most of Professor Dunlap's are. Let us suppose, to continue thinking along these lines, that a visile attempts to judge from his image the beauty of a person or of a scene. That beauty is likely to be appraised only after the selection and exaggeration of certain aspects to which the judge is himself peculiarly sensitive. The auditory beauty, to say nothing of beauty which may appeal to other senses, may be entirely ignored. My own impressions of a famous scientist's recent work are dominated by visual images of certain salient features in his distinguished appearance and a faint auditory image of his booming voice. My judgment upon his work, in so far as I presume to have any, is that he is getting too self-satisfied. But is it his work alone, or it and the images which he has left in me that I am criticising? and my impression of Professor Einstein's almost unique power to enquire about things as if nobody had ever investigated them before is very hard to dissociate from my visual memory of his child-like appearance.

5. *Is the verbaliser caged?*

It should be clear that if this be true, the habitual verbaliser may be caged; often, though by no means always, in a public park instead of in his own garden. Phrases and words may hem him in just as completely as the visile's pictures, if he is the corresponding sort of verbaliser. But he probably realises this less vividly because many other people share his word-images and accept them at their face-value without any bargaining. Yet he and they may be hypnotised by remembered phrases as the visile by remembered appearances. Successful political speakers of a certain kind appear to employ all the nouns in their language which end in 'ation'; co-operation, nationalisation, civilisation, stabilisation, and the rest. The speech is then impressive—to minds like the speaker's. But to the visile those words in such a context offer the thumbed appearance and maybe the repulsive smell of a 'best-seller' after years of service in a circulating library. Oh, for the freshness of clean notepaper and the personal touch of a friend's handwriting! But what if his writing is illegible in parts? This is the shrewd counter-thrust of the verbaliser.

6. *The possibility and importance of a visual logic.*

I once heard a verbaliser say of the thinking processes of a visile, that "visual images come somehow into his mind." That 'somehow' is a challenge. It almost implies that the words which enter a verbaliser's mind do *not* come somehow, or alternatively, that their mode of entry is formal and regularised, while visual images burst in without ceremony. Is the distinction based upon the assumption that the verbaliser's thinking is more likely to be logical? And is this hypothesis in turn supported by the fact that logic—as we know it here and now in the schools—concerns itself chiefly with words?

All these reflections may be justified. But the visile remembers how jealously the verbaliser would distinguish between the writings of John Stuart Mill and the crass auditory associations which litter the speech of the maniac. The visile himself knows the difference between thinking out, in visual images, a practical problem of travel; allowing his mind to play with images for the mere fun of it; experiencing their rapid, jerky transitions in the hypnagogic state, and reflecting upon life in the manner of Miss Dorothy Richardson. These different categories of visual memory—and there are others—make him stand up sturdily for a visual logic. Surely the pictures may follow each other in the mind according to principles of cognitive and affective relevance, cause and effect, subordination and superordination, and the rest. Hogarth's "Rake's Progress" and Fougasse's "Progress of a Wireless Enthusiast" in *Punch*—are they not logically determined? And are there not logicians who have not hesitated to use pictures or diagrams?

And yet there comes the reflection that this visual logic may be only a kind of dream-logic; that perhaps the dream-mechanisms of Freud may operate only with visual imagery, for presumably, most of the dreams which formed the basis of his theory were visual. May it be, as Mr F. C. Bartlett suggested to me, that the way in which visual images get mixed up with each other, and the way in which verbal images come together, need not necessarily have much in common?

The questions seems to be open. In the dreams of a person whose auditory imagery was good and visual imagery bad, E. R. Thompson[1] found one condensation—"a voice like A.'s with a squeak in it like B.'s." Word-condensations in dreams are common enough. There seems too to be no *a priori* reason why condensations from the larger muscle groups should not occur in dreams—if kinaesthetic images really exist. If they do not, it seems unlikely that experiences from widely separated parts of the body should be linked with each other except by dint of many lessons and much practice. This consideration might be useful in the question of kinaesthetic imagery, but the matter is too crude to be developed here.

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[1] This *Journal*, VII, 1914, 310.

A STUDY OF PERFORMANCE TESTS

By FRANCES GAW.

(*From the Industrial Fatigue Research Board*[1].)

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I. INTRODUCTION.

PERFORMANCE tests, as the term is used in this paper, mean brief mental problems, intended primarily to measure intelligence, and to do so in non-verbal terms. These tests, which necessitate the solution of a concrete problem, may be classified on the basis of the materials used, as (1) manipulative, those in which the subject handles and re-arranges actual objects, such as blocks of wood and weights, and (2) non-manipulative, those in which he deals with pictures or other symbolical representations of the objects.

The object of the present investigation was twofold: (1) to analyse the qualities measured by a group of performance tests, and (2) to determine whether this group of tests is standardised satisfactorily for normal English elementary school children. The method followed was to analyse the tests by comparing them with each other and with other tests, by means of observations and introspections given by psychologists, and by consideration of the effect of environment on scores. For the latter purpose a group of canal-boat children was given the performance tests. In order to evaluate the standardisation of these tests for English children, the scores made by 100 pupils in London schools were compared with scores made by the three groups with whom the tests had been standardised, American and Australian children, and (in the case of two tests) men in the U.S. army.

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[1] The test materials for this research were supplied largely by the National Institute of Industrial Psychology.

II. DESCRIPTION OF THE TESTS USED.

A. Performance tests.

In the present study a series of 14 performance tests was used[1], comprising several tests from Pintner and Paterson's scale[2], several tests from the U.S. Army[3] performance scale, and the Porteus maze test[4] in addition. The directions for giving and scoring the tests were compiled in a manual based on Healy's and Porteus' work, and on published data regarding the U.S. Army's and Pintner and Paterson's scales. By means of the tables in this manual a given score on any test may be at once translated into terms of mental age. After finding the mental ages for each of the tests given, the method followed was to take the median of these mental ages, and to compute an I.Q. based on this median. The total range of mental ages given in the table for these 14 tests is from 3 to 18. The tests vary considerably in the range of mental years they cover, however, analogous to variations appearing in the Stanford-Binet scale, where some types of tests are re-inserted at several age levels (as memory for digits), while others appear only at a few (as the fable, or ball and field).

The average chronological age of the children tested in this research was 13·5 years. Twelve of the 14 tests described differentiate mental years at and near this age, while the two others, the Manikin and Goddard Adaptation Board, do not differentiate between mental ages above 8, that is, the maximum score is considered compatible with any mental age at or above 8. Applied to children of 13, such tests serve as a check upon other tests; they have a negative significance, and can be taken into account only if the child fails.

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[1] This series was arranged and first used by the psychologists at the Psychopathic Hospital in Boston, Mass., U.S.A.

[2] Pintner, R. and Paterson, D., *A Scale of Performance Tests.* D. Appleton & Co., N.Y., 1917.

[3] Yoakum, C. S. and Yerkes, R. M., *Army Mental Tests.* Henry Holt & Co., N.Y., 1920.

[4] Porteus, S. D., *Porteus Tests—Vineland Revision.* Publication of the Training School at Vineland, N.J., No. 16, 1919.

It should be here emphasised that to measure a subject's intelligence it is not sufficient to give a single test, or two or three tests; a series is indispensable. Performance tests are based on the principle, made familiar by the Binet scale, of sampling a number of different processes and amalgamating the total results. To do this, it is essential to give a number of different tests, just as in the Binet scale.

The following paragraphs describe the performance tests used in this study:

1. Healy Picture Completion Test I.

This test is a picture of 12 people, mostly children, doing various things—as playing ball, sawing wood, and so forth—in an outdoor scene. Ten holes have been cut in the picture, removing objects essential to the various actions going on. These holes are to be filled by the subject with appropriate insets. He is given a collection of small pictures, among which are included the missing objects, together with other pictured objects, several of which could be put into the large picture without absurdity. For each hole there is but one small picture, however, which fully completes the sense of the large picture. Each hole which is filled correctly receives a certain number of points.

2. Healy Picture Completion Test II.

This is a test similar to the preceding, except that it is more difficult. It consists of 10 parts each representing a scene in a schoolboy's day—for example, he is shown eating breakfast in one part, on his way to school in the next, and so forth. In each part some essential thing is missing and has to be filled in by the subject. The scoring is similar to that of Picture Completion Test I.

3. Manikin Test.

4. Profile Test.

These two tests consist of pieces of wood which, when correctly put together, represent a man, and a man's face in profile, respectively. In the Manikin Test the scoring is based on errors in placing arms, legs and head of the man. In the Profile Test, the scoring is based on the time taken to put the pieces together correctly.

5. Cube Construction Test.

This test consists of three models: two of them wooden blocks 1 × 3 × 3 inches, each made to resemble a composite of nine 1 inch cubes, the third a 2 inch wooden cube, made to resemble a composite of eight 1 inch cubes. Model 1 is painted on the four sides, but not on the upper and lower surfaces, model 2 is painted on the upper surface as well as on the sides, model 3 is entirely unpainted. The subject is given each model separately, together with the number of small cubes necessary for its construction. Some of the cubes are painted and some unpainted, and in each case the subject is to arrange them so that they resemble the given model. Scoring is based on the time and the number of moves or placings to assemble the cubes correctly.

6. Dearborn Formboard.

This test is a formboard containing six different types of figures or insets of simple geometrical shapes. In arranging the insets, there are numerous different combinations possible so that they are all placed in the openings in the board, and so that all openings in the board are filled. The subject is given three problems, in each of which he must work out the minimum re-arrangement of the blocks inside the board necessary to make room for one or more blocks outside the board. The scoring is based on the time and number of moves or placings of the blocks.

7. Porteus Maze Test.

This test consists of seven mazes, graded progressively in difficulty and standardised for chronological ages 3 to 14 inclusive. The score is based on the number of years passed and the number of trials necessary for success at each year.

8. Cube Imitation Test.

In this test, four black 1 inch cubes are placed in a row before the subject. With a fifth cube the examiner taps the four in a given order. There are 12 such patterns of increasing difficulty, which the subject is required to imitate. The scoring is based on the number of patterns correctly imitated.

9. Goddard Adaptation Board.

This test is an oblong wooden board through which four circular holes have been cut. Three of them are about 6·8 cm. in diameter and the fourth about 7 cm. in diameter. There is a wooden block which exactly fits the largest hole. The examiner illustrates that the block will fit only one hole, and then turns the board in five different positions, asking the subject after each change to put the block into the hole where it will fit. The scoring is based on the number of times the first placing of the block is correct.

10. Substitution Test.

In this test the subject is given a sheet on which are printed 50 geometrical forms of five different types in irregular order. These forms appear at the top of the sheet numbered 1, 2, 3, 4, 5. The task for the subject is to fill in the forms with the appropriate numbers, the scoring being based on the time taken, and on the errors made.

11, 12 and 13. Triangle and Diagonal Tests and Healy Puzzle A.

These are small formboards, the recesses of which are to be filled with blocks of various geometrical shapes. Scoring in these three tests is based on the time taken for a correct solution.

14. Goddard Formboard.

This formboard is a large board in which are cut recesses for 10 blocks representing simple geometrical figures. The problem is to put the blocks into the board as rapidly as possible. The score is the time taken in the shortest of the three trials allowed.

B. Additional tests to be compared with performance tests.

For purposes of comparison several tests in addition to the performance tests have been included in this research. These additional tests measure four different qualities or abilities, as follows:

1. *Measurements of intelligence.*

(*a*) Pintner's Non-language Group Intelligence Test[1]. This test is almost unique among group tests in that it is presented entirely in pantomime, by means of gestures, very simple diagrams and drawings. The subject's response is also non-verbal. This test is suitable for adults, as well as children. There is a time limit, and the subject is penalised for errors.

(*b*) I.Q.'s, determined by the Stanford-Binet scale[2], were available for the children referred to in this study.

(*c*) The intelligence of each child was estimated by his school-teacher, and the judgments thus obtained used for comparison.

2. *Tests of mechanical ability.*

(*a*) Stenquist's Assembling Tests of General Mechanical Ability, Series I and Series III[3].

(*b*) Healy's Puzzle Box[4].

Of these tests the first, Stenquist Series I, consists of 10 common mechanical objects, such as an electric bell, a mouse-trap, and so forth, the parts of which are given to the subject to assemble. Series III is similar except that it is much easier. In both these tests the scoring is based upon the number of objects successfully assembled, and upon the total time taken to complete the series.

The Healy Puzzle Box[5] is a wooden box with a glass top and four holes cut in the sides and bottom. It is fastened by a complicated system of cords and rings slipped over pegs inside the box. The problem is to open the box with the aid of a button-hook which may be inserted in the holes. Time taken to open the box determines the score.

3. *Test of constructive ability.*

Kelly's test of constructive ability[6] consists of a number of small wooden blocks and pegs of different sizes and shapes. The subject is told to build whatever he pleases and to do it as well as he can. Hence he must invent his design, as well as carry it out. His score depends on the excellence or merit of the structures that he builds. There are many different levels of merit, each illustrated by a photograph of an actual specimen. The examiner must determine the merit of each structure by comparing it with these photographs.

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[1] Pintner, R., "A Non-Language Group Intelligence Test," *J. of App Psy.*, Sept. 1919.

[2] Terman, L. M., *The Measurement of Intelligence.* Houghton Mifflin Co., 1916.

[3] Stenquist, J. L., *Measurements of Mechanical Ability.* Bureau of Reference, Research and Statistics, Board of Education, N.Y.

[4] Healy, W. and Fernald, G. M., "Tests for Practical Mental Classification," *Psy. Mon.* XIII, 1911.

[5] This test is here used to measure mechanical ability, as previous experience seems to justify its inclusion with tests of this type, rather than with those of general intelligence.

[6] Kelly, T. L., "A Constructive Ability Test," *J. of Ed. Psy.*, Jan. 1916.

In addition to mechanical and constructive ability, manual dexterity was undoubtedly measured indirectly in the Goddard Formboard and in parts of several other tests just described. In these tests, especially those of Stenquist, the subject must not only understand and recognise mechanical relationships; he must also have the dexterity, the deftness, the control of his hands, necessary to put the pieces together.

The children studied in this research were given tests for various special abilities and for specific vocations[1], as well as tests of scholastic attainments[2]. The results of these measurements will be referred to incidentally.

III. RESULTS OF THE TESTS.

Of the two groups of subjects in this research, elementary school children and canal-boat children, the experiments with the former will be described first, beginning with the question of the standardisation of the tests. It should be borne in mind throughout that the American children with whom most of the performance tests were standardised were of a somewhat higher social status than the English children tested. Hence the latter may have been at a slight disadvantage as compared with the American children.

A. Results with elementary school children.

1. *Standardisation of the tests.*

There were 100 children in this group; 52 boys and 48 girls, the average chronological age of the two groups being 13·5. These children were pupils at three elementary schools under the London County Council; the schools had been selected as medium schools, in which the children would be of average ability and of average social status. The 100 subjects, all of whom were to leave school within a few months, were given the tests described in this paper, as part of an experiment in vocational guidance. The group contained no children of scholarship or central school ability as they had been removed by examination at

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[1] See Spielman, W., "Vocational Tests for Dressmakers' Apprentices," *J. of the National Institute of Industrial Psychology*, July, 1923.

[2] Burt's tests for reading, spelling, arithmetic, writing and drawing. See *Mental and Scholastic Tests*, L.C.C. 1921.

11 and again at 13 years. Thus, those who would constitute about the upper 10 per cent. in a group of children leaving school at 14 were not included. All the tests were applied within the same calendar year.

A comparison of the scores on which the norms are based with the scores obtained by the elementary school children is shown in Table A. The first horizontal row of figures in the table shows the norms or the median scores obtained from the individuals with whom these tests had been standardised. (The norms are for boys and girls together.) The second horizontal row of figures shows the median scores which the 100 elementary school children (boys and girls together), attained in these tests[1]. The third row of figures shows how the scores of the English children are distributed above and below the norms. It will be seen that the standardisation is satisfactory for Substitution, Picture Completion II, Triangle, Goddard Formboard, Cube Imitation, Healy A, and Diagonal; too easy for the Adaptation Board, Manikin, Profile and Picture Completion I; too hard for Porteus Mazes, Cube Construction Test, and Dearborn Formboard. This is the result to be expected with the Manikin and Adaptation Board, since, as already stated, their chief significance with subjects above 8 years of age is largely negative, that is, they are only to be taken into account if a child fails on them. The results are rather surprising with Picture Completion I and the Profile Test, particularly the latter, as there seems to be no common toy or scholastic exercise especially familiar to English children which would account for their marked superiority over American children in this test. As regards the Porteus Mazes, it is possible that the author's scoring makes them too difficult for children of 13·5. Burt's norms for London children[2] give average mental ages of 13·2 and 13·8 (mental ages and scores are identical in this test) to children between 13·0 and 14·0 and between 14·0 and 15·0 respectively. Since his group included scholarship and central school children, his results suggest that the low scores here reported are due partly to the standardisation of the tests, but mainly to the comparative inferiority of the group of 100 children tested in this study.

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[1] Two of the tests, the Substitution and the Dearborn Formboard, are not strictly comparable to the others. In the former, wherever mentioned in this paper, mental ages rather than scores are referred to. For Table A, the median mental age was found for the group of 100, and the score for which this mental age stands was given. The records from the Dearborn Formboard are based on scores from 55 subjects only. The apparatus for this test was not available for use with all the children.

[2] Burt, C., *Mental and Scholastic Tests.* London County Council, 1921.

TABLE A. *Comparison of Norms and Scores Attained by 100 Elementary School Children.*

Test	Cube Imit.	Adapt. Bd.	Goddard Fmbd.	Mani-kin	Profile	Healy A.	P.C. I	P.C. II	Triang.	Diag.	Porteus	Subst.	Cube Con.	Dearborn Fmbd.
Norm (expected median score for Chron. age 13·5)	7·5 Points*	5 Points	12·5 Seconds	5 Points	150 Seconds	36·5 Seconds	511·5 Points	60 Points	38 Seconds	31·5 Seconds	13·5 Points	98 Points and Seconds	16·5 Points and Seconds	18·2 Points and Seconds
Elem. sch. children's scores (attained median score by children of 13·5)	8	5	13	5	76·5	49	578	62·2	40	35	12·5	99	12	15
Percentage of subjects (above and below norms)	Above 57	70†	57	87†	79·5	42	69	54	45	48	12·5	46·5	20	14·5
	Below 43	30	43	13	20·5	58	31	46	55	52	87·5	53·5	80	85·5

* Scoring in this test is based on points; in some others on seconds or on both points and seconds as indicated.

† The median and the maximum scores are the same in these two tests.

TABLE B. *Sex Differences in Performance Tests.*

	Cube Imit.	Adapt. Bd.	Goddard Fmbd.	Mani-kin	Profile	Healy A.	P.C. I	P.C. II	Triang.	Diag.	Porteus	Subst.	Cube Con.	Dearborn Fmbd.
Sex differences in median scores	−0·5	0·0	0 0	0·0	−7·0	+0·9	+41·5	+3·0	+25·0	+17·5	0·0	−2·0	−1·0	−0·5
Semi-inter Q. Range B.'s	1·0	0·5	1·5	0·0	32·5	33·5	38·5	8·7	13·5	23·0	0·7	2·5	3·5	3·5
G.'s	1·0	0·5	1·5	0·0	52·5	35·5	63·5	14·0	25·0	22·5	0·7	2·5	3·5	2·5
Av.	1·0	0·5	1·5	0·0	42·5	34·5	51·0	11·3	19·2	22·7	0·7	2·5	3·5	3·0
Sex differ. as % of P.E.	−50	0	0	0	−16·5	+2·6	+81·4	+26·6	+130·0	+67·1	0	−80·0	−38·6	−16·6
Sex differ. as multiple of S.D.	−33	0	0	0	+·11	+·02	+·56	+·18	+·88	+·45	0	−·54	−·26	−·11
% of boys' in excess of girls' median score	31	0	48·1	0	46·1	53·8	78·8	61·5	77	67·3	36·5	42·3	42·3	45·1

Sex differences in performance tests.

It is important to determine whether there are differences between the sexes either in the scores on separate tests or in the sum total of accomplishments. Taking the latter point first, and comparing the average performance I.Q. for the boys (·96) with that for the girls (·93), the difference is seen to be almost negligible. A study of the individual tests, however, reveals marked differences between the boys and girls, as shown in Table B. The figures in the last row of the table indicate that the differences between the sexes are striking in six tests; in Picture Completion I, Triangle and Diagonal, in which the boys excelled, and in Cube Imitation, Porteus Substitution and Cube Construction, in which the girls excelled. It is of interest that the girls are superior in tests of non-geometrical form relationships and of learning ability, while the boys excel in tests of geometrical form relationships and of reason. What connection tests of form relationships may have to mechanical ability is not determined, but it is worthy of note that various measurements of this ability include tests of this type, especially tests involving geometrical forms. It may be that the differences between boys and girls in performance test scores simply illustrate two of the sex differences in intelligence tests found by Burt, Terman and others (1) that while girls excel in a linguistic, rather academic type of test, boys are superior in a more perceptual, sensori-motor type of test, and (2) that girls usually have been found to excel on memory tests, boys on reasoning. However, these differences between the sexes as shown in performance tests have been almost entirely ignored heretofore.

In the tests of mechanical and constructive ability, sex differences were also apparent. Of even greater interest than the numerical differences shown in Table B, were the outstanding differences between the boys and girls in type of response. These differences were especially noticeable in the Kelly test, in which the girls tended to build homely, domestic structures, such as a 'house' or a 'kitchen table,' while the boys very often copied some object of a mechanical type such as a 'crane' or a 'motor.' Both boys and girls sometimes built fanciful or fantastic structures also.

Differences between two so-called types of subjects in non-verbal tests.

In addition to the undoubted differences between the sexes, the results on performance tests and the other non-verbal tests revealed two tendencies in reaction, or methods of expression, a linguistic and a non-linguistic. Several rather extreme instances of these two tendencies which appear to be largely independent of sex, were observed. For example, one child whose scores were above the average in the performance tests and tests of mechanical ability, whose manual dexterity (as measured incidentally by the mechanical tests), and 'constructive ability' in non-linguistic terms were good, nevertheless made a low score for her age in the Binet test, in tests of scholastic ability and of creative imagination in linguistic terms. This child's good record in the dressmaking tests, as compared with her poor record in linguistic studies, served as a further indication of her non-linguistic bent. The home report as to her hobbies, leisure pursuits and general interests was also in accord with the study made of her by means of the tests.

Translation of scores into mental ages.

One part of the standardisation of performance tests is the translation of scores into the mental ages for which these scores are the equivalent. In the present group of performance tests, the range of scores for some mental ages was found to be so wide as to lower the discriminative capacity of certain tests. This range of scores is not the same for all tests, and for all mental ages in the same test. For this reason it was necessary, in order to arrive at a more accurate evaluation, to use scores rather than mental ages, in finding medians and in calculating correlations. In all data, therefore, scores are always used, except in one or two cases where it is specifically stated otherwise, and except in working out I.Q.'s.

The median scores and the average scores for each of the performance tests are compared in Table C. The largest differences between medians and averages are found in the Profile, Healy A, Picture Completion I, Triangle and Diagonal Tests, in all of which there are some exceptionally large and small values which probably disturbed the average.

2. *Factors measured by performance tests.*

The second question to be considered in this section is:—what qualities are measured by performance tests? The facts here dealt with might be thought of as (*a*) statistical, and (*b*) non-statistical.

(*a*) *Statistical results.* The questions to be answered are: have the tests a factor in common, and if so, what is the nature of this factor, is it a special ability or is it identical with general intelligence?

To throw light on the first of these two questions the inter-correlations of scores on the 14 performance tests were calculated by the Product-Moment formula, and have been arranged in the form of two hierarchies. The probable errors for boys range from ± ·07 to ± ·12; and for girls range from ± ·08 to ± ·12. The correlations, although low, are not inconsistent with the existence of a common factor, which might be measured in differing degrees, by all but three of four of the performance tests.

There are negative inter-correlations with several tests—especially the Dearborn Formboard and Triangle for the group of boys, and the Dearborn Formboard, Healy A and Manikin for the group of girls. It may be significant that the boy who makes the highest score on the Dearborn Formboard makes the lowest I.Q. in the Stanford-Binet test, and the lowest but four in the performance tests. As this test was found to be satisfactory in the U.S. Army, conclusions can hardly be drawn as to whether a special ability is tested on the basis of this study.

The inter-correlations between the performance tests are somewhat lower than might be expected. This result could be accounted for if the tests were found to have low reliability[1]. The fact that the inter-correlations among the performance tests are rather low makes it very necessary to use not one or even two or three, but a number of tests to arrive at a mental age, just as the Binet scale combines several tests.

In the negative inter-correlations there are marked differences between the sexes; and one of the most significant things to be noted from the tables is that the tests which inter-correlate most highly for the boys are not those which give the highest inter-correlations for the girls.

Since the hierarchies, so far as they go, are consistent with the assumption that there probably is a common factor the next step is to determine as far as possible what this common factor is. To answer this second question, the correlations between performance tests and other measurements of intelligence, and tests of mechanical and constructive ability were calculated and are shown in Table D.

Correlations between performance tests and other estimates of intelligence.

There is an obvious relationship between the performance tests and other measurements of intelligence. Two correlations between the performance and Binet tests were calculated for each sex; first, a correlation

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[1] It was impossible to give the tests twice in this research. However, with a group of ten-year-old children reliability coefficients were found as follows:

Boys: 13 tests ·76. Girls: 13 tests ·54.

The group of 13 tests included all those used in this study, except the Porteus test. The reliability of several of the individual tests was found to be low. (See report on performance tests to the I.F.R.B.)

TABLE C. *Median vs. Average Scores.*

		Cube Imit.	Adapt. Bd.	Goddard Fmbd.	Mani-kin	Profile	Healy A.	P.C. I	P.C. II	Triang.	Diag.	Porteus	Subst.	Cube Con.	Dearborn Fmbd.
Boys (52 cases)	Med.	7·5	5	12	5	79·5	41	608	66·2	27·5	27·5	12·5	99	11	15*
	S.I.Q.R.	1	·5	1·5	0	32·5	33·5	38·5	8·7	13·5	23	7	2·5	3·5	3·5
	Av.	7·4	4·6	12·5	4·8	101·9	64·3	580	62·4	36·7	49·3	12·1	103·9	12·4	14·6
	S.D.	1·6	·6	1·7	·5	81·1	48·7	97·7	14·4	25	50·1	1·3	2·7	4·3	3·9
Girls (48 cases)	Med.	8	5	12	5	72·5	50	566·5	59·2	52·5	45	12·5	97	12	15·5†
	S.I.Q.R.	1	·5	1·5	0	52·5	35·5	63·5	14	25	22·5	·7	2·5	3·5	2·5
	Av.	8·1	4·7	13·4	4·9	116·5	68·8	528	61·1	68·3	70·6	11·9	100·4	11·7	15·4
	S.D.	1·4	·6	2·1	·4	96·3	48	89	18·8	47·5	67·8	1·5	2·6	4·9	3 1

* 31 cases. † 24 cases.

Inter-correlations of Performance Tests. 52 Boys.

	Dia-gonal	Goddard Fmbd.	Cube Con.	Substi-tution	P.C. I	Profile	P C II	Mani-kin	Healy A.	Cube Imit	Adapta-tion Bd	Porteus	Tri-angle	Dear-born Fmbd
Diagonal		·52	·37	·36	·19	·43	·37	·39	·18	·17	·20	·17	·13	–·19
Goddard Formboard	·52		·21	·36	·34	·37	·32	·11	·29	·19	·09	·18	–·05	·10
Cube Con.	·37	·21		·30	·03	·16	·22	·33	·37	·23	·09	·12	·22	·03
Substitution	·36	·36	·30		·35	·01	·18	·10	·33	·15	·29	·08	–·12	–·35
P.C. I	·19	·34	·03	·35		·00	·34	–·01	–·05	·33	·16	·13	·13	·18
Profile	·43	·37	·16	·01	·00		·35	·36	·03	–·02	·31	·13	·18	–·25
P.C. II	·37	·32	·22	·18	·34	·35		–·05	·15	·18	·28	·11	–·19	–·28
Manikin	·39	·11	·33	·10	–·01	·36	–·05		·10	–·20	·13	·08	·34	·04
Healy A.	·18	·29	·37	·33	–·05	·03	·15	·10		·27	·01	·02	–·05	·05
Cube Imitation	·17	·19	·23	·15	·33	–·02	·18	–·20	·27		·26	·15	–·17	·13
Adaptation Bd.	·20	·09	·09	·29	·16	·31	·28	·13	·01	·26		–·17	·12	–·31
Porteus	·17	·18	·12	·08	·13	·13	·11	·08	·02	·15	–·17		·02	–·21
Triangle	·13	–·05	·22	–·12	·13	·18	–·19	·34	–·05	–·17	·12	·02		–·11
Dearborn Formboard	–·19	·10	·03	–·35	·18	–·25	–·28	·04	·05	·13	–·31	–·21	–·11	
	3·29	3·03	2·68	2·14	2·06	2·06	1·98	1·72	1·70	1·67	1·46	·81	·55	–1·23
Performance I.Q.'s	·71	·54	·53	·55	·45	·45	·50	·26	·38	·46	·46	·25	·18	–·33
Binet I.Q.'s	·23	·32	·33	·35	·18	·19	·57	·05	·08	·27	·21	·52	·07	–·01

Performance I.Q.'s and Binet I.Q.'s: ·41

Inter-correlations of Performance Tests. 48 *Girls.*

	Substitution	Cube Con.	Profile	Cube Imit.	G Formboard	P.C. II	Triangle	Porteus	P C I	Adaptation Bd.	Diagonal	Healy A.	Manikin	Dearborn Fmbd.
Substitution		·43	·24	·34	·43	·38	·69	·09	·38	·31	·08	·07	–·13	–·31
Cube Con.	·43		·47	·25	·38	·33	·15	·31	·27	·23	·16	·10	–·07	–·45
Profile	·24	·47		·26	·25	·16	·22	·35	·17	·25	·10	–·08	–·01	·14
Cube Imit.	·34	·25	·26		·23	·31	·27	·42	·56	·20	–·08	–·03	–·09	–·24
Goddard Formboard	·43	·38	·25	·23		·36	·27	·28	·19	·12	–·09	–·05	–·07	–·11
P.C. II	·38	·33	·16	·31	·36		·11	·08	·37	·02	–·04	·07	·00	·03
Triangle	·69	·15	·22	·27	·27	·11		–·27	·03	·22	·09	·22	–·06	·08
Porteus	·09	·31	·35	·42	·28	·08	–·27		·07	·12	·12	–·04	·17	·01
P.C. I	·38	·27	·17	·56	·19	·37	·03	·07		·07	·04	–·02	–·16	–·48
Adaptation Board	·31	·23	·25	·20	·12	·02	·22	·12	·07		·02	–·12	–·09	·10
Diagonal	·08	·16	·10	–·08	–·09	–·04	·09	·12	·04	·02		·24	–·06	·44
Healy A.	·07	·10	–·08	–·03	–·05	·07	·22	–·04	–·02	–·12	·24		–·13	·05
Manikin	–·13	–·07	–·01	–·09	–·07	·00	–·06	·17	–·16	–·09	–·06	–·13		·29
Dearborn Formboard	–·31	–·45	·14	–·24	–·11	·03	·08	·01	–·48	·10	·44	·05	·29	
	3·00	2·56	2·52	2·40	2·19	2·18	2·03	1·72	1·49	1·45	1·01	·29	–·40	–·45
Performance I.Q.'s	·66	·54	·49	·51	·37	·47	·37	·48	·48	·40	·13	·22	·02	·02
Binet I.Q.'s	·44	·40	·30	·39	·24	·38	·13	·29	·43	·35	·20	–·10	–·06	–·21

Performance I.Q.'s and Binet I.Q.'s: ·49

TABLE D. *Comparison of Performance I.Q.'s with*

1. *Other Estimates of Intelligence.* 2. *Tests of Mechanical Ability.* 3. *Test of 'Constructive Ability.'*

	Estimates of Intelligence				Tests of Mechanical Ability			Test of 'Constructive Ability'
	Performance I.Q.	Binet I.Q.	Non-language group test	Teachers' estimates	Stenquist I	Stenquist III	Puzzle-box	Kelly test
*Average Score**								
Boys	·95	·94	107	—	54·7	83·4	4′ 26″	96·8
Girls	·93	·97	—	—	—	70·8	—	72·7
Correlation with Performance I.Q.'s								
Boys: fourteen tests	—	·41	·51	·38	·29	·31	–·05	·06
six tests	—	·55	(30 subjects)	—	(30 subjects)	—	(30 subjects)	—
Girls: fourteen tests	—	·49	—	·43	—	·16	—	·21
six tests	—	·67	—	—				
Correlation with Binet I.Q.'s								
Boys: fourteen tests	·41	—	·40	·74				
six tests	·55	—	(30 subjects)	—				
Girls: fourteen tests	·49	—	—	·73				
six tests	·67	—	—	—				

* Where number of subjects is not given in this table there are 52 boys and 48 girls, total groups tested.

in which the total group of performance tests was used, and second, a correlation based on the six tests found to correlate most highly with the Binet I.Q.'s. That these I.Q.'s correlate more highly with the teachers' estimates than the performance I.Q.'s, and less highly with the non-language group test of intelligence was to be expected. Both the Binet test and the teachers' estimates have a marked linguistic bias; while the non-verbal group test makes no use of language, and further resembles the performance tests in that form relationships enter into several parts.

In addition to the correlations with the Binet tests shown in Table D, coefficients were calculated between each individual performance test and the I.Q. obtained from the group of performance tests as a whole, and between the individual performance tests and the I.Q. obtained from the Binet test. The degree to which the performance tests are inter-correlated one with another is almost exactly the degree to which they are correlated with the Binet I.Q. The correlations between the two orders—that is, the order for Binet I.Q.'s and individual performance tests, and the order for performance I.Q.'s and individual performance tests—is as high as ·84 for the girls. Although it is lower with the boys, ·68, this is very largely due to the peculiar character of the Porteus test which might very naturally be expected to correlate highly with the Binet scale and only to a low degree with the manipulative tests. Thus the tests (namely, Cube Construction, Substitution, Goddard Formboard, P. C. I, P. C. II and perhaps Profile) which correlate best with general intelligence—as measured by the Binet scale—also correlate best with the central 'performance' capacity. This seems to furnish strong corroborative evidence that the central factor measured by performance tests is largely general intelligence.

It should be here emphasised that to evaluate non-linguistic tests entirely by comparing them with linguistic is a very questionable procedure. While it is true that a high correlation between a non-verbal test and the Binet scale would prove that the former measures general intelligence, yet, the converse, a lack of correlation between a non-verbal test and the Binet scale, does not prove that the former does not measure intelligence.

Correlations between performance tests and tests of mechanical and constructive ability.

There appears to be some relationship between the Stenquist tests and performance tests for boys; less relationship for girls. The Puzzle

Box[1] seems to be a very different type of test from the Stenquist tests[2] although the boys in this group did well with the Puzzle Box compared with other English boys of the same age[3]. Their record is somewhat lower than that reported by Healy for American boys[4]. To judge by the Stenquist tests Series III, the boys do much better than the girls. As even this form of the test was obviously too hard for many of the girls, they were not given the two more difficult mechanical tests.

The results with the Kelly test are rather misleading. The scoring for the girls was necessarily done largely on a subjective basis, as there were often no 'equivalent structures' in the photographs by comparison with which Kelly planned the test to be scored. Nevertheless it is probably a fair assumption that the boys' apparent superiority is genuine, since mechanical ability, in which they excelled, appeared helpful in this test. The scoring of the Kelly test is somewhat unsatisfactory even for boys so that probably little weight should be attached to the correlations involving this test.

(*b*) *Non-statistical results.* Certain results which cannot be shown statistically were brought out in this study. Chief among these—as far as the analysis of the performance tests is concerned—were the introspections of seven psychologists (four women and three men), who have taken different tests in this series. According to these introspections, the tests may be classified as measuring primarily reasoning, memory and imagery. Among tests involving principally reasoning should be mentioned Picture Completion Tests I and II, in which both analysis and synthesis are used[5], and the Porteus test in which analysis and synthesis are employed in a different way; in 'planning ahead' and in finding a solution for each stage of the problem correct in relation to the succeeding stages as well as to the preceding. Imagery of kinaesthetic and visual types were those most frequently reported, the former being mentioned as helpful in 10 of the 14 performance tests.

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[1] As the leather straps used in the Puzzle Box stretch when the weather is damp, it was not altogether satisfactory as a test, since not all the children used it under identical conditions.

[2] The Stenquist tests are perhaps not altogether fair to English children, since some of the 'common' mechanical objects are less common in England than in America, where the tests were standardised.

[3] See an unpublished thesis, "Studies in Practical Ability," by M. MacFarlane, Bedford College.

[4] Healy, W., *The Individual Delinquent.* Little, Brown & Co., 1915.

[5] The principal quality measured by these tests has been called 'apperceptive ability' by Bronner, who has analysed results obtained from normal and delinquent children. See "Apperceptive Abilities," *Psy. Rev.* XXVIII, July, 1921, and also "The Apperceptive Abilities of Delinquents," *J. of Delinquency*, VII, Jan. 1922.

In addition to reasoning, memory and imagery, all of which might be considered expressions of general intelligence, a special ability, manual dexterity, was also measured, although probably in only one test [the Goddard Formboard] of this series[1]. It was further suggested that perception of form relationships might also depend on a special ability[2], a possibility which has already been noted in connection with the negative correlations found with the Dearborn Formboard. Moreover, as previously mentioned, the fact that the boys excelled the girls in tests of form, and also excelled in the mechanical tests, into which form relationships appear to enter, might be taken as a further indication that these two types of tests have a common factor, perhaps dependent on a special ability.

Temperamental qualities could be said to be measured directly in two tests only of those considered in this paper; the Kelly test, in which are shown the subject's originality, initiative, 'dominance of purpose[3]' and desire to make a structure 'workable,' and the Porteus test in which ability to take forethought, to 'plan ahead' and to work with deliberation, as opposed to impulsiveness, are measured. However, performance tests, even more than most other intelligence tests, furnish valuable opportunities for observing temperamental qualities indirectly. The material used is to most children more novel than Binet's material, a fact which may, on the one hand, arouse more interest, or on the other hand, bring out more self-consciousness or timidity. Since so many performance tests are scored on speed, as well as accuracy, or on speed of reaction alone, it follows that the subject's method of working—resulting from temperamental as well as intellectual factors—is more dependent in these tests than in the Binet scale on ability to work under pressure, and to withstand distraction by unessentials in the material or by noises in the room

B. Results of the performance tests with canal-boat children.

Thirty-four canal-boat children between the ages of 6 and 13 were given the performance tests for the purpose of judging the effect of an abnormal environment on success in the tests. Twenty-seven of the same children had previously been given the 'abbreviated' Stanford-Binet scale, Burt's graded spelling test, and Ballard's tests in the speed of reading, adding, and subtracting[1]. The results with these measurements are compared with results from performance tests in Table E. The 'educational ratio' is the ratio between a child's chronological age and the total of his scores on the scholastic tests. By the first performance I.Q. is meant that calculated from all the tests given to a child; by the second is meant that calculated from all the tests given, except the Substitution. This test, since it involves speed of writing, seemed unfair to canal-boat children, to whom, with their very limited education, writing is a slow and extremely difficult process.

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[1] For other series, containing performance tests in which manual dexterity apparently enters to a greater extent see Healy, W. and Fernald, G. M., "Tests for Practical Mental Classification," *Psy. Mon.* No. 2, XIII, 1911, and Dearborn, W. F., Shaw, E. A. and Lincoln, E. A., *A Series of Formboards and Performance Tests of Intelligence.* Harvard Mon. in Education, Series 1, No. 7, Sept. 1923.

[2] See Lowe, G. M., Shinsberg, M. E. and Wood, M. W., "Further Standardization of Construction Tests A and B." *J. of App. Psy.*, Sept. 1924.

[3] This quality is to be scored for each subject, according to Kelly's instructions for the test.

The results shown in Table E seem to justify the conclusion that the performance tests are less influenced by environment and by lack of schooling, than is the Binet scale. Assuming that the distribution of performance I.Q.'s is analogous to that of Binet I.Q.'s, the average of these children's records in performance tests is but five points below the Binet I.Q. found by Burt[2] for normal children in a poor school; though with the Binet tests they are just below the borderline for mental deficiency, with the performance tests they are near the upper limit for the 'dull and backward'—almost, but not quite, normal.

TABLE E. *Performance Tests with Canal-Boat Children.*

	Performance I.Q.'s	Binet I.Q.'s
Average	27 children (1st I.Q.) ·79 (2nd I.Q.) ·82	76 children ·70 27 children ·69
	With Binet I.Q.	With Perf. I.Q.
Correlation with other measurements of intelligence	27 children (2nd I.Q.) ·68	27 children (2nd I.Q.) ·68
Correlation with educational ratio	18 children ·26	26 children ·71 18 children ·58
Correlation with chronological age	27 children (2nd I.Q.) ·27	76 children −·75 27 children +·09

Whether performance tests are influenced at all by environment is a question which cannot be answered from this single study; since, either canal-boat children as a group may be backward due to heredity, or performance tests, although better adapted than the Binet scale to these children, nevertheless do not do them justice. The superiority of the performance I.Q.'s over the Binet I.Q.'s, however, is great enough to suggest that non-verbal tests form the best method of studying children from such an abnormal environment as that of the canal-boat child.

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[1] See Gordon, H., *Mental and Scholastic Tests among Retarded Children*, Board of Education, Education Pamphlet, No. 44, 1923.

[2] Burt, C., *Mental and Scholastic Tests.* London County Council, 1921.

The correlation between Binet and educational tests is ·58, between performance and educational tests, ·26. Although the group which took all three tests is very small, this result suggests that with children from so limited an environment, educational opportunity plays a far larger part in scores with the Binet tests than in those with the performance tests.

The canal-boat children exhibited a marked lack of mental discipline in the performance tests, an inability to work together, to react quickly and to adapt to a new situation promptly, which can perhaps be accounted for by their lack of opportunity for an ordinary school life, and their aloofness from the usual social contacts. Considering specific results, their fantastic misinterpretations of pictures in Picture Completion tests I and II, and their unusually marked pleasure in colours should be mentioned.

That these children do not conform to the usual standards for even mental development, and hence do poorly on intelligence tests may be more a reflection on the too narrow application of the tests than on the ability of the children.

IV. SUMMARY AND CONCLUSIONS.

1. A group of 14 performance tests constructed in America have proved directly applicable to children in English schools, although the standardisation of several of these tests needs some modification for use with English children.

2. It appears probable that, with three or four possible exceptions, the performance tests here used are to a large extent, but in differing degrees, tests of some central mental capacity; and so far as statistical inferences are allowable from so small a group, the correlations obtained are consistent with this view.

Judged by the average degree of the inter-correlation, the best tests of this central 'performance' capacity are Cube Construction, Substitution, Goddard Formboard, Picture Completion Tests I and II, and possibly the Profile Test. The least effective are the Dearborn Formboard, the Manikin, the Adaptation Board, Healy A, and the Triangle Test.

3. There is presumptive evidence that the central factor measured by these tests is largely, if not wholly, general intelligence. The tests which correlate best with intelligence as measured by the Binet scale, are much the same as those enumerated above as correlating well with the central 'performance' capacity. The Porteus Maze tests also correlate with the Binet scale; and it may be noted that the Picture Completion

tests correlate rather better with Binet than the correlations with the central 'performance' factor would suggest. A special ability, manual dexterity, is measured, too, in at least one test, the Goddard Formboard. Some of the results also suggest that perception of form, as involved in a number of the tests, may be a special ability.

4. It seems that performance tests measure intelligence in somewhat different terms from those in which it is measured by such tests as the Binet-Simon scale. This may be due either to the presence of a specific factor (or factors) in the performance tests, to a specific factor in the Binet tests, or to both.

It follows, therefore, that linguistic and non-linguistic or performance tests supplement each other in very valuable and necessary ways. And it appears that with subjects from so very limited an environment, as that of the canal-boat children, performance tests furnish a better means of judging intelligence than do the Binet tests.

5. The average degree of inter-correlation between these non-linguistic tests is somewhat lower than that obtained in a series of tests of a linguistic type. It is, therefore, even more necessary with performance tests to use not one, or even two or three tests, but a number. To test what may be called intelligence in non-linguistic terms by one performance test alone, a formboard, a picture completion test, or the scale of Porteus Mazes, can lead to only very precarious results.

6. There appear to be, among individual children, specific tendencies or types, some subjects doing better throughout in linguistic tests and others in non-linguistic.

7. There are, at any rate in some of the performance tests, very well-marked sex differences. Generally speaking, the boys seem to be better than the girls, particularly in tests requiring perception of form. In some of the tests, however, the sex difference appears comparatively negligible, for example in Picture Completion II, the Goddard Formboard and Porteus Mazes among the tests correlating most highly with 'performance' capacity as here determined; and the Manikin, the Adaptation Board, and the Dearborn Formboard among the tests which show the lowest correlations with this 'performance' capacity.

This difference between the sexes has curiously been almost entirely ignored by the investigators who have done the most work with these tests. But it is evident that for several of them it may be necessary to construct separate norms for boys and girls respectively.

(*Manuscript received* 7 *November*, 1924.)

CAN LABORATORY EXPERIMENTS ON OUTPUT THROW LIGHT ON PROBLEMS OF INDUSTRIAL FATIGUE?

By H. M. VERNON.

1. *Introductory* (pp. 393–394).
2. *Variability of Output in Individual Subjects* (pp. 395–399).
3. *The Number of Subjects Employed* (pp. 400–401).
4. *Differences in Character of Laboratory Tests and Industrial Operations* (pp. 402–404).

1. *Introductory.*

The speed and accuracy with which the workers in various occupations perform their manual operations have often been subjected to detailed investigation, in order to obtain light on various problems of industrial fatigue, but usually it is found very difficult to obtain the particular conditions of experiment which are desired, and to control the conditions of production sufficiently closely to get reliable results. In consequence, attempts have been made, and are still being made, to obtain the desired information by means of laboratory experiments. In such experiments the conditions of production can be strictly controlled, and might on this account be thought to yield more reliable results; but nevertheless there are considerable grounds for thinking that they can throw very little light on the fatigue of manual workers working under industrial conditions.

The laboratory experiments made in recent years have been carried out by one to six subjects for a period of not more than eight weeks, and the work has generally lasted only 5 or 6 hours a day for not more than four days per week. Hence the subjects were able to work at maximum speed, as they knew that if they over-exerted themselves they had plenty of time for recuperation in non-working hours. Genuine industrial workers, on the other hand, almost always work a regular 44 to 48 hour week, which means an 8 to 9 hour day on all week days except Saturday, and in many cases they know that they will continue at the same task for months and years to come. However much they desired it, therefore, it would be impossible for them to work at such high pressure as laboratory subjects. It is true that most of the workers, when they are paid at a piece rate, do work at their maximum speed, but this speed is so regulated that the weekly 44 to 48 hours of work can be performed at a steady rate for month after month, with no intervals for recuperation other than Sundays and bank holidays. The available evidence suggests that industrial workers unconsciously succeed in maintaining an even speed of production throughout working hours. Striking evidence in support of this conclusion has recently been obtained by Wyatt[1] in the cotton industry. The hourly output of 7 to 25 weavers, working in five different sheds, was investigated for a six-month period (April to September), and in every instance it was found that the mean output kept constant, to within 3 per cent., throughout the working day, except for the first half-hour of the work spells. This was in spite of the fact that the weaving was carried out under most trying atmospheric conditions, as the sheds were artificially humidified, and the wet bulb temperature was often well above 70° F. The lower output at the beginning of the work spells is a well-known phenomenon which is almost invariably observed in industrial occupations. It is usually due in part to the workers wasting time in starting their work, but it is also due to a lack of the enhanced skill which comes from practice, or 'warming up.'

The fatigue effect, though it existed in the weaver's output curves, was barely recognisable, and the curves obtained in many other occupations likewise show very little sign of fatigue. A simple way of testing the influence of fatigue is to compare the rate of production during the afternoon work spell with that in the morning spell, and I showed[2] that in a large number of occupations (with a 7½ to 9 hour working day) the rate was equal, on an average. When, however, the working hours were 10 per day, the afternoon output was 2 per cent. less than the morning output, and when strenuous muscular work was performed, it was 6 to 16 per cent. less.

Evidence has recently been obtained[3] in a number of other industries which suggests that the hourly output is nearly steady throughout the working day, provided that the initial and final quarter (or half) hours of the work spells are excluded, for the output at the end of the work spells usually falls off owing to time being lost in preparations for departure. However, for reasons indicated below, no great stress can be laid on the reliability of the output curves mentioned, or indeed on almost any of the published records, except the above-mentioned data of Wyatt.

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[1] Wyatt, S., *Report No. 23 of Industrial Fatigue Res. Board*, 1923.

[2] Vernon, H. M., "*Industrial Fatigue and Efficiency*," p. 23, 1921.

[3] Vernon, H. M. and Bedford, T., *Report No. 25 of Indust. Fatigue Res. Board*, p. 15, 1924.

2. *Variability of Output in Individual Subjects.*

If direct observation be held to indicate that experienced industrial workers tend, as a whole, to maintain an even rate of production throughout the working day, it seems highly probable that this is the most efficient method of work, and it is of interest to determine how far *individual* workers succeed in attaining it. A good deal of evidence on the subject was obtained by the writer in conjunction with T. Bedford[1], and typical samples of it will be described. All of it relates to women and girls who were on piece rates and were thoroughly experienced, as they had been two years or more at their job.

In the operation of labelling small packages, a process which took about 4 seconds, a group of seven girls was observed on three occasions for a period of four days each time. As the girls do not always settle down steadily to their work on Monday mornings, and are sometimes rather disturbed on Friday afternoons owing to its being pay day, the output was measured on the Monday afternoon and the Friday morning, and the three intermediate days. The average hourly rate of production, except in the initial and final quarter-hours of the work spells, is shown in Fig. 1, and it will be seen that in period (*a*) it was fairly steady. Subsequent to this set of observations the girls were given a 10-minute rest pause at 10.20–10.30, during which they had a cup of tea, and a fortnight later their output was again determined. Speed of production was apparently unaffected by the rest, as the total output came to the same average, after multiplying the 9.45–10.45 output values by 6/5,

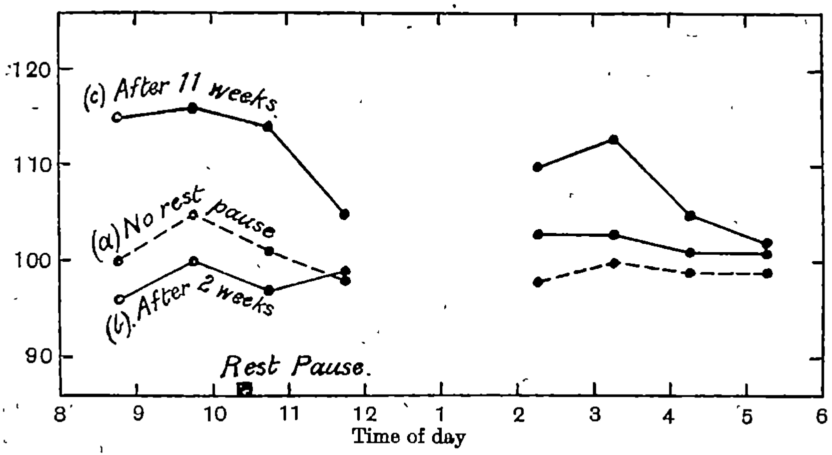

Fig. 1. Output of girls labelling packages.

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[1] *Loc. cit.*

and it will be seen that curve (*b*) corresponds fairly well with curve (*a*). After the rest pause had been in force for 11 weeks, however, the mean rate of production was found to be 10 per cent. greater than before, but it will be seen from curve (*c*) that the hourly output, instead of being nearly steady as before, fell off considerably towards the end of each work spell. This change of form was not due to the introduction of the rest pause, but to the fact that there was a sudden burst of very hot weather. This tired the girls so much that they were unable to maintain, throughout the day, the improved speed of production which they had unconsciously acquired as the result of the introduction of the rest pause. Hence these results show how unreliable hourly output records may be unless they are continued for such a period as to neutralise the effects of temporary changes in the conditions of work.

The output records of one of the labellers are shown in Fig. 2, and it will be seen how far they depart from the theoretical constancy. The variation in the slope of the recorded curves is rather greater than the average, whilst the variability of the hourly outputs from the mean output in each period is rather less than the average, but the curves recorded may be accepted as a fair sample of the results obtained. We may conclude, therefore, that whatever the *average* hourly output of individual workers, their output from day to day shows very little consistency. During one week it may gradually rise during a work spell, during the next week it may gradually fall, or it may vary irregularly from day to day in the most capricious manner. The variations observed were not due to irregular amounts of time being lost in collecting material, in taking voluntary rests, etc., as such time was small in amount (4 mins. per hour), and kept fairly steady.

The standard deviation of the hourly outputs of each individual from the mean observed in each period was calculated, and the means of the deviations are recorded in the table. As the hourly output averaged 100,

Occupation	Mean hourly output	Standard deviation
7 girls labelling (without rest pause)	100	7·3±1·2
7 girls labelling (rest pause for 2 weeks)	100	6·2±1·0
7 girls labelling (rest pause for 11 weeks)	110	8·7±1·5
6 girls making starch cartons	100	10·7±1·0
4 girls padding caps	100	13·6±1·1
4 girls stamping out tin discs	100	14·0±1·1

the deviations are really coefficients of variability, and it will be seen that they averaged about 7. This was smaller than that observed in the other three operations recorded, when they averaged 10·7–14·0.

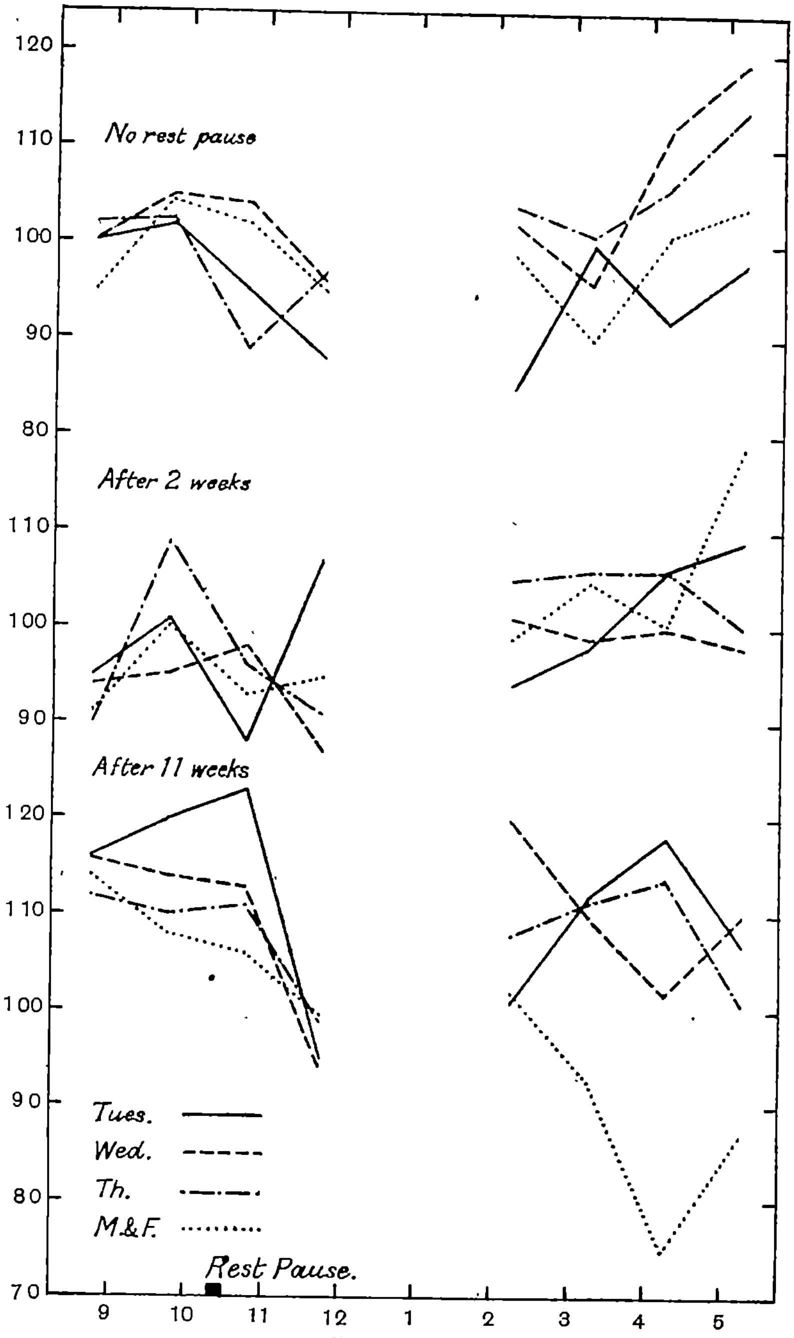

Fig. 2. Output of one labeller.

The operation showing the greatest variability of output was that in which girls punched out discs from sheets of tin by means of a stamping press. In order to eliminate irregularities due to the occasional stoppage of the machines for adjustments, only actual working time was considered in estimating output. Fig. 3 shows the hourly output of one girl for four days, and it will be seen that there is no sort of consistency, many outputs being 20–30 per cent. above or below the mean, and falling or rising rapidly during the work spells. Yet the curve at the bottom of the figure, which shows the mean output of four girls for four days, is fairly steady. In this instance the output was measured for the whole of the 5 hours of the morning spell and the 4 hours of the afternoon spell, without eliminating the initial and final quarter hours.

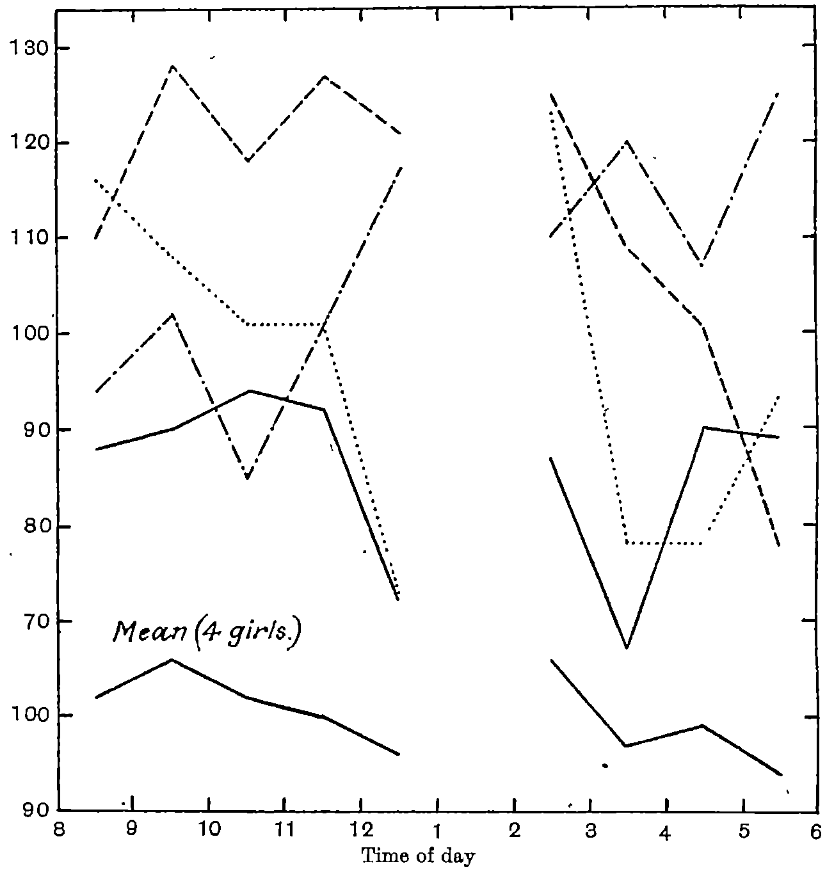

Fig. 3. Output of girl working stamping press.

The other two operations studied consisted of cap padding, in which the girls inserted a disc of cardboard or linoleum into metal caps (for tin canisters) at 1 second intervals, and the 'making' (opening and pasting on a paper strip) of starch cartons, which took 3 seconds. The variability of output was almost as great as in the tin-punching operation, and the output curves were almost as inconsistent as those figured.

As already stated, all the girls whose outputs were investigated were thoroughly experienced, and presumably they would be more likely to attain a steady output than relatively inexperienced laboratory subjects. Such subjects would tend on some days to start working at such a rapid rate that they could not possibly maintain it throughout the work spells, and the next day, fearing a repetition of such a result, they might start at too slow a rate, and after a time find that they could quicken up considerably without undue fatigue. On other occasions they might change backwards and forwards from hour to hour without hitting off the right mean, and it is only after they had continued on their daily task for weeks and months that one could place much reliance on their output records. Supposing that laboratory subjects had the energy to put in a genuine 44 hour week of work for several months on end, so as to eliminate the effects of inexperience and of working at too high pressure, would their output records then be comparable to those of industrial workers? By no means. Industrial workers almost invariably work in close proximity to other workers, with whom they hold frequent conversation, and they are often interrupted by the need of fetching fresh material, and of carrying away the articles produced. I have shown[1] that in a large number of monotonous occupations these breaks usually occur at the rate of four or five per hour, and they aggregate about 7 minutes per hour in actual duration. They must have a powerful influence in reducing the monotony of the work, and must materially affect the speed of production. Laboratory subjects, on the other hand, usually work in isolation, and so arrange their work that they have no interruptions of any kind. If, in order to make the conditions of work more similar to those found in industry, they arrange to work in proximity with other subjects with whom they hold converse, and to introduce other interruptions, there is still another very important factor which differentiates them from industrial workers, viz. intelligence. The mentality of most laboratory workers is such that they would find it almost impossible to continue on a monotonous repetition task for months on end, whilst the average industrial workers (or at least the women) seldom if ever complain of boredom, and many of them prefer monotonous tasks to those requiring thought and initiative.

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[1] Vernon, H. M., *Report No.* 26 *of Industrial Fatigue Res. Board*, 1924.

3. *The Number of Subjects Employed.*

Another objection to laboratory experiments lies in the difficulty of getting sufficient subjects to yield a reliable result. Often the experiments are confined to a single subject, and they are rarely made on more than three subjects. Yet the individual results obtained show such striking disagreements that no generalised conclusion can be arrived at. For instance, Wyatt[1] got three subjects to perform tests (*a*) in simple addition, (*b*) with a comptometer, and (*c*) with a dynamometer, for two 2½ hour work spells on four days a week for six weeks, and he found that though the output curves in the adding test were fairly consistent, those in the other two tests showed scarcely any agreement in the various subjects. I myself made a series of type-writing tests in conjunction with two other subjects (M. D. Vernon and C. G. Warner), and observed very great inconsistencies. The tests consisted in striking 2, 3, 4 or 8 keys of the typewriter in definite sequence with the digits of one or both hands for a half-hour interval. After a practice period, each of the five tests was performed ten times, and it was found that, for instance, whilst one subject performed one of the tests 44 per cent. faster than another subject, and three other tests respectively 24, 20 and 9 per cent. faster, he performed one of the tests 1 per cent. *slower*. The third subject showed smaller inconsistencies. During the last 6 minutes of each 30 minute test, the subject was asked questions in the simple multiplication table, question and answer being performed at maximum speed. This mental test had no effect whatever on the typing speed of one subject, whilst in another subject it increased the speed 3 per cent. In the third subject it increased it 4 per cent. in two of the tests, and diminished it 2 per cent. in three of them. The typing errors made by two of the subjects remained nearly steady throughout, including the final 6 minutes when the mental test was performed, but in the third subject they increased gradually, and during the mental test they doubled their previous number.

It will be gathered, therefore, that the results of the typing tests were so inconsistent as to be almost valueless, but they did yield one result which was so consistent that it can be accepted with fair confidence. The multiplication test was performed each day in the absence of typing, as well as in its presence, so as to obtain a control. The multiplications were read out from a long list, and question and answer together took only about a second. The speed with which the test was performed improved steadily, and in a fortnight increased 50 per cent. However, the speed was *invariably* a good deal smaller when typing was simultaneously performed, the average retardation observed in the three subjects being 25, 22 and 16 per cent. respectively. In spite of the increasing speed with which the test was performed, the percentage retardation produced by the typing was nearly constant, for it averaged 19, 21, 20, 22, 20 and 17 per cent. in consecutive intervals of 2–3 days.

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[1] Wyatt, S., *op. cit.* p. 24, 1924.

One of the subjects showed the same degree of retardation in all the five typing tests, but the other two showed a greater retardation the more complex the test. The striking of two keys with the digits of one hand caused an 18 per cent. retardation; of three keys, a 21 per cent. retardation; of four keys, a 23 per cent. retardation; of four keys (two with each hand) a 25 per cent. retardation, and of eight keys (four with each hand) a 29 per cent. retardation.

In addition to the typing, the three subjects performed a test which closely resembled that performed by girls engaged in assembling the component parts of bicycle chains. After a practice period, the test was performed for half an hour on 14, 20 and 24 occasions by the three subjects, and it was found that two of them performed it at a nearly steady rate during the first 24 minutes, whilst the third subject showed an 8 per cent. improvement of speed. In the last 6 minutes, when the multiplication test was simultaneously performed, the subjects *increased* their speed of chain assembling by 1, 4 and 6 per cent. respectively. Just as in the typing observations, the speed of the mental test was considerably retarded by the simultaneous mechanical work, the reduction amounting to 12, 14 and 30 per cent. in the three subjects.

These results suggest that when industrial workers are engaged on monotonous repetition work involving quick movements, their mental processes are slowed and dulled. Hence they are not so liable to think of the monotony of their labour or to magnify their grievances as they would do when they are employed on monotonous work involving only slow and occasional movements, *e.g.* those that are often required in controlling semi-automatic machinery. However, even this deduction is uncertain, for it is to be remembered that both the mechanical and the mental tests were being performed at maximum speed. It may well be that if they had been performed at a leisurely rate, they would scarcely have interfered with one another at all.

4. *Differences in Character of Laboratory Tests and Industrial Operations.*

Though there is no inherent reason why the tests performed in a laboratory should not closely resemble actual industrial operations, the fact remains that, owing to the difficulty of getting the required material in sufficient quantities, or the appropriate machinery, it is customary to select laboratory tests which have little or no direct relation to industry. In such a case, can the conclusions arrived at be applied to industrial processes with any sort of conviction? For instance, Muscio[1] performed an 'aiming' test and a 'pendulum' test continuously for periods of 2½ hours, and he found that he steadily increased in accuracy, in spite of the increasing fatigue incident to the performance of the tests. If, however, he quickened the speed at which he performed the aiming test, his accuracy got less in proportion to the quickening, so he considered his observations to indicate that the principal cause of the variations observed in the frequency of industrial accidents from hour to hour is rate of work rather than fatigue. Such a conclusion is not justified, for he did not continue his tests long enough. On the single occasion on which he performed the pendulum test for 3½ hours, his accuracy diminished substantially during the last hour, and mechanical tests performed by other investigators sometimes show a much earlier onset of inaccuracy. For instance, C. G. Warner, at the suggestion of the writer, typed a memorised passage (which took him about 87 seconds) at maximum speed for a 3-hour period. He sometimes took rests (at the rate of 6 minutes per hour), and sometimes not. As a mean of the 20 tests made after the practice effect had nearly been eliminated, it was found that the number of typing errors made began to increase slowly after 45 minutes' typing, and at the same time the typing speed began to diminish. Presumably this earlier onset of a fatigue effect was due to the much quicker movements required in the typing than in the aiming and pendulum tests, as the keys were struck at an average rate of over five per second. Hence it is impossible to argue from the results obtained in one test to those obtainable in others, and still less to actual industrial operations.

Direct observations on inaccuracy in industry, as measured by the amount of spoilt work, have given very contradictory results. Pieraccini[2] observed a rapid increase in the errors made by type-setters during the course of their work spells, and so did Link[1] in his investigations on shell inspectors. On the other hand, Ryan and Florence[2] observed a decrease of spoilt work during the course of the work spells of women engaged on two metal work operations, whilst a third group showed a decrease in the morning spell and an increase in the afternoon spell. The writer, in conjunction with T. Bedford, made observations on a group of 17 girls engaged on the rather hot and laborious work of soldering ½ and 1 gallon tins. After soldering, the tins were tested for tightness, and about a third of them showed small leakages which required soldering up. The accompanying figures indicate that the percentage of tins re-soldered, and also the output of the girls, remained fairly steady during the course of the working day. The girls put in a 9-hour day of work (8–1 and 2–6), except on Saturdays, but the data relating to the initial and final half-hours of the work spells are omitted.

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[1] Muscio, B., *Report No.* 19 *of Indust. Fatigue Res. Board*, 1922.

[2] Pieraccini, *Proc. First Internat. Congr. Indust. Diseases*, Milan, 1906.

Hours	8.30–9.30	9.30–10.30	10.30–11.30	11.30–12.30	2.30–3.30	3.30–4.30	4.30–5.30
Output	97	101	98	98	104	107	101
Percentage of tins re-soldered	30	27	37	35	30	32	30

Hence it follows that the hourly variations in accuracy observed in one occupation do not necessarily hold for others, and the only reliable method of finding out what will happen in a given occupation is to make direct observations on the occupation itself.

Owing to the difference in the character of laboratory tests from industrial operations, and the fact that the former are usually performed at maximum speed without regard to subsequent fatigue, it is probable that such tests are of very little value for the investigation of rest pause effects. Wyatt[3] got a subject to perform an addition test for two work spells of 2½ hours each, and he found that if a 15-minute pause was introduced in the middle of each spell, the output increased 19 per cent., whilst two 7½-minute rests per spell gave an 11 per cent. increase. On the other hand, C. G. Warner, when performing the 3-hour typing test above mentioned, was able to increase his actual speed of typing only 2 per cent. when he took 6 minutes rest per hour (either 1 minute in 10, 3 minutes in 30, or 6 minutes in 60), and accordingly his total productivity, over the 3-hour spell, was reduced some 8 per cent. A better imitation of industrial conditions was achieved by Isabel Burnett[4], who chose four unemployed girls as her subjects. She got them to work at cross-stitching on canvas for two 3-hour spells a day, on four days a week. The experiment was continued for eight weeks, and during the last three weeks it was found that output was at a maximum on the days when no rest pauses were taken, and no talking allowed. On the days of unorganised rest pauses 10 per cent. less work was done, and when three 5-minute rests were interposed in each spell, the output was 16 per cent. less. However, the days when no rests or talking were allowed were very much disliked, and it is obvious that these conditions could not be insisted on in industrial work. Hence the results of this experiment lose their value in proportion as the conditions depart from those observed in industry, and still more is this true of the other two investigations mentioned.

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[1] Link, H. C., *Journ. Indust. Hygiene*, I, p. 233, 1919.

[2] Ryan, A. H. and Florence, S., *Journ. Nat. Inst. Indust. Psychol.* I, p. 193, 1923.

[3] Wyatt, S., *Report No. 25 of Indust. Fatigue Res. Board*, p. 21, 1924.

[4] Burnett, Isabel, *Journ. Nat. Inst. Indust. Psychol.* II, p. 18, 1924.

The chief conclusion suggested by the above discussion is obvious. In order to obtain information which can be directly applied to industrial problems, it is necessary to collect this information strictly under industrial conditions. It is true that it is often difficult to find a factory where the desired conditions are in existence, and many of the data collected have to be rejected because of unavoidable variations in the conditions of work and for other causes; but frequently it is possible to secure the co-operation of the management in keeping working conditions constant, without at the same time altering their industrial character.

(*Manuscript received* 27 *May* 1924.)

MENTAL TESTS FOR THE SELECTION OF UNIVERSITY STUDENTS

By AGNES L. ROGERS.

Smith College, Northampton, Massachusetts, U.S.A.

I. *The Object of the Investigation* (p. 405).

II. *The Tests used and Method of Application* (pp. 405–407).

III. *The Validity of the Thorndike Intelligence Examination* (p. 407).
  (*a*) *Reliability of Academic Marks* (pp. 408–409).
  (*b*) *Reliability of the Tests* (p. 409).
  (*c*) *Predictive Power of the Tests* (pp. 410–412).
  (*d*) *Comparison of the Predictive Power of the Tests, High School Marks and Entrance Examinations* (pp. 412–413).

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V. *Conclusions* (p. 415).

I. THE OBJECT OF THE INVESTIGATION.

THE chief purposes of this investigation were to determine the value of group psychological tests as a means of selecting students and in the event of their proving sufficiently trustworthy to establish satisfactory standards for women. The present report will cover only the first of these problems.

II. THE TESTS USED AND METHOD OF APPLICATION.

The Intelligence Examination devised by E. L. Thorndike was used. Enumeration of the names of the tests contained in it will indicate its general character.

Part I comprises the tests: Following Directions, Disarranged Sentences, Arithmetical Computation, Arithmetical Reasoning, Information, Synonym—Antonym, Practical Judgment, Completion of Number Series, Analogies, Discrimination of the Largest and Smallest Numbers in a Series, Detecting Absurd Statements, Logical Inference, and Recognition of Spatial Forms.

Part II includes the following tests: Reading—its interpretation, Sentence Completion, Pictorial Completion, Pictorial Analogies, Algebraic Computation, Geometrical Pictorial Analogies, Physics Information and a General Information test embracing literature, history, mechanics, sport and a wide variety of subjects.

Part III contains a Reading Test similar to the reading test in Part II and a Sentence-Completion Test.

It will be noted that some of these tests have been widely used. Considerable information has been accumulated concerning the amount of interdependence between the mental functions they involve. For several of them the degree of correspondence between the abilities measured and the mental character called 'General Intelligence' has likewise been determined. This particular group of tests had already been tried in principle, though not in its present form, in selecting candidates for cadetships in the United States Air Service and for the Students' Army Training Corps of Columbia University. In both instances it had yielded sufficient differentiation of ability and satisfactory correlation with intelligence. In its present form the examination is much more extensive and thorough than the Binet-Simon series of tests for adults, the Stanford Revision, the Army Scale Alpha or the intelligence examination used by the United States Air Service. Tests of linguistic and mathematical abilities predominate, though some pictorial material demanding perceptual intelligence is included. The tests do not measure to any extent such phases of intelligence as those involved in the ability to comprehend and manipulate things and the capacity to understand and manage persons. As, however, the purpose of this study was to ascertain how valuable psychological tests would be as one element in the requirements for admission to universities and as the abilities concerned in learning from lectures and books and in dealing with ideas and symbols predominate in academic work rather than the capacities to understand and control things or people, it was to be expected that the tests used would prove highly indicative of the kind of intelligence a university course requires and would forecast with considerable accuracy future achievement with academic work.

Two hundred and fifty-four first year women students of Goucher College, Baltimore were tested in October, 1919. Since the four years required for graduation in American colleges were completed June, 1923, it is now possible to analyse the test results with reference to their power to predict academic achievement. The average age of the group at the time of testing was 18 years 5 months, the standard deviation being

12½ months. Part I, Form C, and Part III of the examination were applied in two of the regular English class hours, each occupying 30 minutes of actual working time. Part I, Form E, and Part II were given on the same day in a special period lasting 90 minutes. Thus the total duration of the actual testing was 2½ hours. Before the examination, ten minutes were devoted to familiarising the students with the tasks to be performed. This not only helped to allay any excitement that might be present, but gave a better measure of the individual in the mental functions tested, preventing emphasis of the ability to follow printed directions rapidly and equalising opportunity also to some extent. This fore-exercise was given with duplicate forms of Part I. Six instructors in the department of English, who had been trained for the work, administered the tests. Each instructor had charge of his own students, the sections varying in size from 25–30. The examination was held three weeks after the beginning of the academic year.

III. THE VALIDITY OF THE THORNDIKE EXAMINATION.

To determine the value of the tests as an instrument for forecasting future academic achievement, it was necessary to select a satisfactory measure of the latter. The criterion adopted in this study was academic marks. It is unnecessary to defend the use of these at length, as they in fact do decide whether a student may continue working towards a degree in colleges of liberal arts in America. Low or failing marks in several subjects compel the undergraduate to withdraw. In the case of Goucher College, such marks are based not only on regular half-yearly examinations in each subject studied, but also take into account numerous shorter examinations varying from 10 minutes to an hour, held at the discretion of instructors, as well as upon written papers of the essay description and upon laboratory work in the case of those subjects who require it. Marks for the first year thus include from four to six records by as many different instructors. By the end of the senior year, the academic rating represents the composite result of approximately 30 separate records. Few of these are made by the same instructor, in general not more than three for any student. Written examinations vary in type as well as in other respects. Some are comparatively objective and reliable in character. These may be constructed in the form of a sentence-completion, a recognition test, or a true-false test. The last is widely used. It consists of a series of statements, opposite which the student records in a way involving little or no handwriting, for example, using plus and minus signs, whether these are true or false. In the case of such examinations several of the factors which tend to make academic records unreliable are absent or reduced in influence. In general, however, it cannot be claimed that academic marks provide an entirely satisfactory means of evaluating the tests of intelligence, as they still remain imperfect in reliability.

The method of recording a student's success in any course in its final form recommended for use is that grades should follow the normal frequency curve. Thus in required subjects, 3 per cent. of a class should obtain an A grade, 22 per cent. B, 50 per cent. C, 15 per cent. D, and 10 per cent. E or F, while in elective courses the same number of A, B, and C grades are recommended but the number of D's is increased to 22 per cent. and the number of E and F grades is decreased to 3 per cent. A denotes very superior work, B superior achievement, C average work, D inferior achievement, E attainment such that the student will not pass unless further work is done and F complete failure. The method used to change these literal grades into quantitative form, was to assign to A the value 5, to B the value 4, to C the value 3, to D the value 2, and to F the value 1. The grade E was in all cases changed to D or F on the basis of further work done.

(*a*) *Reliability of Academic Marks.*

Table I indicates the degree of reliability found for Goucher College marks treated in the way described for this group of 254 women, as determined by the amount of correlation between those obtained in one year and those of other years. The formula used was the standard Bravais-Pearson one.

TABLE I. *Coefficients of Correlation between Academic Marks in Successive Years. Goucher College, Class* 1919–23.

First year and second year marks	$r = \cdot 75 \pm \cdot 02$
First year and third year marks	$r = \cdot 66 \pm \cdot 03$
First year and fourth year marks	$r = 57 \pm \cdot 04$
Second year and third year marks	$r = \cdot 74 \pm \cdot 02$
Second year and fourth year marks	$r = \cdot 65 \pm \cdot 03$
Third year and fourth year marks	$r = \cdot 72 \pm \cdot 03$

The arithmetic mean of the above inter-correlations is ·68. This agrees with results found elsewhere. From similar investigations at Columbia College[1] and Smith College[2] we conclude that academic marks tend to yield a self-correlation of about seven-tenths. It follows that we need not expect a higher degree of correspondence between them and intelligence measures. One sample of academic achievement could not, save by chance, agree more closely with measures of intellectual power, than with another sample of itself. It is important to bear this in mind in evaluating the intelligence examination in terms of its agreement with this criterion.

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[1] Dean of Columbia College and Director of Columbia University Admissions, *Annual Reports*, 1918–22.

[2] Rogers, David Camp, "Intelligence Examinations and College Entrance," *The Smith Alumnae Quarterly*, XIII, no. 1, November, 1921.

(b) *Reliability of the Tests.*

For the same reason the predictive power of the examination will depend upon its own reliability. In an earlier investigation the writer[1] showed that the two halves of Part I of the examination, then known as the Thorndike Test of Mental Alertness, yielded for 98 students at Goucher College in their fourth year a reliability coefficient of ·76 and for 182 students in their first year a reliability coefficient of ·66. For the whole of Part I the reliability would be about ·80. The author of the Intelligence Examination has stated that the entire examination has the unusually high reliability of ·99 for unselected 18-year-olds. In this investigation we are dealing with a group of women who have survived the severe selective process of elementary and secondary education and who have already been admitted to an institution for higher education on the basis of considerable evidence of their possessing sufficient intelligence to succeed with the work required for graduation. Our results are consistent with Thorndike's statement[2] that for the restricted range of college students the reliability coefficient would be ·70 and certainly not under ·60. More recently Wood[3] has presented data from the application of the examination twice to the same group of normal school students. There is evidence that such a group is less homogeneous than students in a college of liberal arts[4]. He states that a reliability coefficient of ·85 was obtained and that all the data available agree in indicating that the Probable Error of Estimate of true scores from scores on one form of the examination is not greater than 3·1 score points.

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[1] Rogers, Agnes L., "Mental Tests as a Means of Selecting and Classifying College Students," *Journal of Educational Psychology*, XI, pt. 4, pp. 181–192, April, 1920.

[2] Thorndike, E. L., "The Reliability and Significance of Tests of Intelligence," *Journal of Educational Psychology*, May, 1920.

[3] Wood, Ben C., *Measurement in Higher Education*, p. 47.

[4] Gambrill, B. L., *Twenty-first Year Book of the National Society for the Study of Education.* "VIII. Some Administrative Uses of Intelligence Tests in the Normal School," p. 232.

(c) *Predictive Power of the Tests.*

The validity of the tests was determined by the amount of correlation between the test scores and academic marks. Table II presents the coefficients for four consecutive years of academic work.

TABLE II. *Correlation between Thorndike Intelligence Examination and Academic Marks for the Four College Years of the* 1919–23 *Class.*

Year	Coefficient of correlation	No. of students
1	$r = \cdot 61 \pm \cdot 02$	254
2	$r = \cdot 45 \pm \cdot 04$	152
3	$r = \cdot 37 \pm \cdot 05$	139
4	$r = \cdot 38 \pm \cdot 03$	132

As we would expect the correlation decreases, since the elimination of poor students tends to make the student body more homogeneous and thus to decrease the amount of correspondence between test scores and academic marks. This, however, does not entirely explain the decrease. When we calculate the interdependence between test scores and academic marks for the four years, taking into account only those students who continued in college throughout the complete course, we see that still the decrease in amount is considerable. Table III presents evidence for this.

TABLE III. *Coefficients of Correlation between the Thorndike Intelligence Examination and Academic Marks calculated only for those graduating in the* 1919–23 *Class.*

Year	Coefficient of correlation	No. of students
1	$r = \cdot 56 \pm \cdot 04$	152
2	$r = \cdot 43 \pm \cdot 04$	
3	$r = \cdot 36 \pm \cdot 05$	
4	$r = \cdot 38 \pm \cdot 05$	

It seems as if the mental abilities measured by the tests were more in evidence in the academic work of the first year than of any subsequent year. Industry and interest in academic subjects may count more towards success in the later years, or more mental adaptability may be necessary in the first year than in succeeding years. That interest is the most potent factor in decreasing the amount of correspondence between test scores and academic marks is most probable. In the first two years the student's time is largely devoted to the study of required subjects, demanding a variety of abilities. For example, a modern language, two sciences, English, philosophy, psychology and biblical literature are required of every bachelor of arts at Goucher College. The tendency is to fulfil these requirements as early as possible. In the last two years the student specialises in the field of her greatest interest and attainment. Consequently zeal is likely to be a stronger influence and is probably more potent in determining academic marks in the last years.

Table II indicates substantial correspondence between test scores and academic marks. It has been noted above that the criterion used has definite limitations. Kelley[1] has pointed out that the maximum correlation possible between two measures of a variable is the product of the square roots of their reliability coefficients. Since academic marks had a reliability of approximately seven-tenths and the Thorndike Examination of about ·85, we need not expect a closer prediction of academic achievement from test scores than ·76. Even if the examination were a perfect instrument, the maximum possible correlation that could be obtained would be ·83. Obviously there are other factors that attenuate the correlation. Temporary disturbances of health, emotional excitement and worry cause disagreement between test standing and college standing to some extent. The economic factor too is likely to enter in. Some students earn part or all of their expenses and it is fairly certain that an excessive burden of this kind lowers a student's academic rank. In the case of women who live at home there are often serious encroachments upon their time. Again moral qualities such as the willingness to exert effort and to persist at a difficult task are perhaps the most significant factors. Earlier (school) training also affects academic success.

To corroborate the suspicion that these were causes of the discrepancies found between students' rating on the intelligence tests and standing in academic work, interviews were arranged with students whose intelligence scores were at variance with their academic marks. In some cases no explanation was forthcoming, but in over half the cases it was possible to find adequate reasons for lack of agreement. For those whose academic success was less than the tests would have predicted, the reasons were as indicated above—defective health, home duties, over-indulgence in social amusements or lack of interest in the subjects studied. In one or two instances, the student stated that low standing was due to lack of sufficient encouragement or pressure and that she knew she could do better academic work.

Similarly in the case of students whose work was average but whose intelligence score was very low, satisfactory explanations were found. Temporary indisposition at the time of the Intelligence Examination was most frequent. Emotional disturbance was sometimes cited. Excitement due to the fact that time limits are imposed flusters the slow-but-sure-thinker and in general it would seem as if the tests applied favour those quick to react.

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[1] Kelley, T. L., *School and Society*, IV, 34, 71, July 1, 8, 1916.

(*d*) *Comparison of Predictive Power of the Tests, High School Marks and Entrance Examinations.*

The value of the tests cannot be estimated satisfactorily without reference to the purposes in view and to other methods of realising them. Comparison was therefore made of the Thorndike Intelligence Examination with the long-established methods of admission in American colleges. The method of certification on the basis of High School records is most widely used. Colleges of liberal arts specify the subjects that must have been studied in High School, the amount of time that must have been devoted to them and the minimum marks that must have been attained. Where close articulation between the secondary school and the college or university exists, as where there is a state system of education from kindergarten to university, admission at the university is obtained on the satisfactory completion of a High School course. With the exception of some prominent universities and colleges in the East, the certificating system is the recognised method of admission. These older institutions have adopted certain elements of the certificating system, but they combine with it entrance examinations of the familiar type in specified subjects or groups of subjects.

We shall, therefore, compare the predictive power of the Intelligence Examination with such information as is available on the predictive value of High School records and entrance examinations. Table IV assembles the correlations obtained from the use of these three means of selecting candidates, where the criterion for estimating their relative merit has been academic grades for the freshman year only.

Evidently this 3-hour intelligence examination can forecast a student's success in college as accurately as any instrument now in use. It can predict achievement in the work of the freshman year in college as closely as High School records covering four years or college entrance examinations occupying 12–14 hours. It is not, moreover, a duplicate of these, but adds valuable supplementary evidence. Success with such tests depends much less on available good instruction than in the case of High School marks or entrance examinations. It reduces the in-

TABLE IV. *Comparison of Correlations of Academic Grades for the Freshman Year with Thorndike Intelligence Scores, High School Records, and Entrance Examinations.*

Thorndike intelligence examination Class		High school Records		College entrance examinations	
Rogers, A. L. 1919	$r = \cdot 61 \pm \cdot 02$	Lincoln[3]	$r = \cdot 69 \pm \cdot 02$	Lincoln[3]	$r = \cdot 47 \pm \cdot 03$
Rogers, A. L. 1920	$r = \cdot 55 \pm \cdot 04$	Dearborn[4]	$r = \cdot 81 \pm \cdot 01$	Thorndike[6]	$r = \cdot 43$
Rogers, A. L. 1921	$r = \cdot 51 \pm \cdot 03$	Smith[5]	$r = 48$	Rogers, D. C.[7]	$r = \cdot 40 \pm \cdot 05$
Wood[1]	$r = \cdot 65$	Wood[1]	$r = \cdot 45$	Rogers, D. C.[7]	$r = \cdot 50 \pm \cdot 03$
Colvin[2]	$r = \cdot 53$			Wood[1]	$r = \cdot 43$
Bridges[8]	$r = 40 \pm \cdot 07$			Bridges[8]	$r = \cdot 48 \pm \cdot 07$

[1] Columbia University: *Report on the Use of Intelligence Examinations in Columbia College*, 1922, p. 10.

[2] Colvin, S. S., "Psychological Tests at Brown University," *School and Society*, x, pp. 27–30.

[3] Lincoln, E. A., "The Relative Standing of Pupils in High School, in Early College, and in College Entrance Examinations," *School and Society*, v, pp. 417–20.

[4] Dearborn, W. F., "The Relative Standing of Pupils in the High School and in the University," *Bulletin, University of Wisconsin*, No. 368.

[5] Smith, F. O., "A Rational Basis for Determining Fitness for College Entrance," *University of Iowa, Studies in Education*, I, p. 30.

[6] Thorndike, E. L., "The Future of the College Entrance Examination Board," *Educational Review*, XXXI, pp. 470–83.

[7] Rogers, D. C., *Smith College Report to Board of Admission*, September, 1921.

[8] Bridges, J. W., "The Value of Intelligence Tests in Universities," *School and Society*, xv, pp. 295–303.

equalities of educational opportunity to a minimum. In this way the facts it furnishes are important aids to decision, as regards candidates for entrance who appear to an admission Board on the basis of entrance examinations or High School records to be doubtful cases.

IV. VALUE OF THE TESTS FOR FORECASTING FAILURES AND SELECTING GIFTED STUDENTS WITH IRREGULAR OR DEFICIENT PREPARATIONS.

The usefulness of psychological tests in selecting a student body depends particularly upon their power to discover in advance those candidates for entrance who will fail to graduate rather than on their general predictive power. The earlier such persons are discovered the better. Teachers will thereby save much fruitless effort and the individual will be protected from needless discouragement and frustration. A test may have substantial predictive power yet fail to make a clear division between students who cannot graduate and those who can. Thus a test or group of tests may be better adapted to discriminate between the very superior and the average student than to distinguish the inferior from the average. Such an intelligence examination adapted to select able students whose preparation has been irregular or deficient would of course be a valuable asset for higher institutions of learning. It has been shown, notably by Proctor[1] that men who had served in the war and had an A + rating on the Army Scale Alpha, admitted to Leland Stanford University in spite of defective scholarship, obtained an average of 64 points above the general average for all the students of their year. Those who had a B + rating reached the general average. One man who had had only five years of schooling before entering the university made "better than a B average on two quarters' work." There is something to be said in favour of waiving entrance requirements in the case of students of very superior capacity who left school prematurely, spent several years in non-academic pursuits and decided afterwards to work towards a degree. Can the Thorndike Examination serve either of these two purposes? Can it discover in advance students liable to fail in academic work? Can it select gifted students whose preparation has been irregular or deficient, but whose intellectual ability is so great that deficiencies can be repaired and successful mastery of academic work gained? The following table presents relevant data:

TABLE V. *Distribution of Scores on Thorndike Intelligence Examination for Various Groups. Goucher College.*

Scores on Thorndike examination	All students	Eliminated for poor work	Graduates with distinction	Entering on probation
Under 30	1	0	0	0
30–39	4	4	0	1
40–49	19	2	0	6
50–59	51	7	0	23
60–69	85	7	2	33
70–79	55	0	2	15
80–89	25	1	4	5
90–99	13	0	2	2
100 and over	1	0	1	0
A.M.	67·2	54·7	81·8	63·9
S.D.	12·8	11·3	14·1	10·7
Number	254	21	11	85

Table V shows that intellectual ability alone does not determine stay in college. The distribution of scores for those eliminated because of unsatisfactory work is skewed towards the lower end of the intelligence scale, but there are students of superior mental capacity who did not graduate. It is noteworthy that 50 per cent. of those dismissed on account of poor work were in the lowest quartile on the Thorndike Examination and 40 per cent. were in the next to the lowest. Only one student dismissed for low marks was above the average on the Thorndike Examination. That student had entered on probation as she did not have the usual preparation. It is significant that all students scoring over 60 on the tests who withdrew before graduation because of low academic marks had had irregular or deficient preparation for university work. The arithmetic mean of the intelligence scores of the 21 students asked to leave owing to their unsatisfactory achievement was 54·7, that for the group as a whole was 67·2.

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[1] Proctor, W. M., "Intelligence Tests as a Means of Admitting Special Students to College and Universities," *School and Society*, XVI, pp. 408, 471–76.

On the other hand, if we attempt to set a lower limit for admission, students who succeeded in graduating would have been refused entrance. Of students scoring below 40, 1 received the degree, of 24 scoring below 50, 12 received the degree, of 75 below 60, 24 finally graduated. From these facts we infer that the chances of success are less where the intelligence score is low. On the basis of intensive study of individual cases all we can say is that any student in good health at the time of the examination, who has exerted average effort and whose score does not exceed 50, is unlikely to graduate. The tests thus cannot be used alone to predict academic success, but can best serve by offering useful supplementary evidence.

A similar limited application can be made of the examination in the selection of students of superior capacity whose preparation for university work has been irregular or deficient. Table V shows that 85 students were admitted on probation. Their intelligence scores ranged from 40 to over 90. Twelve were dropped for defective work and of these all but one were below the average on the Thorndike Examination. It is impossible, however, to establish a critical score for such students. The requirement, for example, that students entering on probation should have a score of not less than 50 would have avoided only one failure of the 12 that occurred and would have prevented 29 persons graduating who actually succeeded.

V. CONCLUSIONS.

The following conclusions may therefore be drawn:

1. The Thorndike Examination can in 2½ hours forecast success with university work as accurately as High School records covering four years and as college entrance examinations occupying 12 hours.

2. The Examination can be used best as a supplement to existing methods of admission particularly to aid decision in doubtful cases.

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PUBLICATIONS RECENTLY RECEIVED

Travaux du Laboratoire de Psychologie Experimentale de l'Université de Cracovie. Edités par W. Heinrich, Directeur du Laboratoire. Paris: Librairie Félix Alcan. 1923. Prix 25 fr.

This very interesting record of research is prefaced by an essay by the editor on the problems of method in psychology. The bulk of the book is divided into three parts. The first of these deals with experiments in spatial perception, the second with research dealing with the problems of attention, and the third contains a single paper dealing with plethysmographic curves in relation to mental work. The first and second parts each contain a general editorial introduction; the various special papers are as follows: "La localisation en profondeur des images doubles," by I. Zając; "Les courbes de repère de la vision monoculaire dans la lumière homogène," by M. Boniecka; "Sur la jonction de la membrane du tympan," by W. Heinrich; "Recherches sur l'acte de lecture d'après les résultats de M. Falski"; "Recherches sur les rapports entre la perception et la reproduction des images d'après les recherches de M. Cyrus Sobolewska"; "Note sur l'acuité de la perception visuelle dans la vision péripherique," by C. Bańkowska; "Le travail mental et la courbe plétysmographique," by H. Trzcińska. The volume contains much solid and admirable work.

Abriss der Psychologie. Von G. E. Müller. Gottingen: Vandenhoeck und Ruprecht. S. 124. Preis 3.8 (geheftet), 4.8 (gebunden).

In this brief but brilliant summary of psychology, Dr G. E. Müller deals with many important and long-standing problems. His general order of exposition does not contain anything novel, for he begins with a study of sensations, and passes on by way of after-sensations, images, temporal and spatial perception, movement, attention, feeling and affect, to a brief final study of self-consciousness. But there is no problem that he touches that he does not treat clearly and with a view to recent experimental work. To every section he adds a brief and interesting list of bibliographical references.

Das Gefühls- und Willensleben. Von Richard Müller-Freienfels. Leipsig: Johann Ambrosius Barth. 1924. S. x + 404. Preis 10.50.

The author explains that this is the first volume of the work which he proposed to call *Grundzüge einer Lebenspsychologie*, the second volume of which deals with *Das Denken und die Phantasie*. He adds, however, that the first volume may be regarded as complete in itself. The term *Lebenspsychologie* is used because, while the writer agrees that careful experimental investigation must furnish the fundamental facts for psychological theory, he wishes to emphasise the view that the activity of consciousness in relation to objects with which traditional psychology is largely concerned, must be studied in its intimate relationship to an activity which remains unconscious, or non-conscious and which must be regarded as wider in its scope and more fundamental in its significance. Further, 'Life' is for him a "specific category which cannot be demonstrated in inorganic nature, an essential form of thought (Denkform) without which organic and conscious life cannot be understood."

It will be evident from this that the volume is written in the interests of general theory rather than with a view to the thorough exploration of any realm of fact.

The introduction deals with the foundation of a vitalistic psychology. The 'self' is said to be the essential explanatory principle of psychology. The author then discusses fully the basis of the life of affect and will, and in pursuance with his general position does this by a study of the various characteristics of 'attitude' (Stellungnahme). First the basic characters of attitudes are discussed, and then the complications which arise in the course of development. A third part of the book consists of applications and expansions of the theories put forward in the first two parts. Here the section on laughter and the humorous frame of mind is particularly interesting.

Der Ausdruck Musikalischer Elementar-Motive. Von Dr KURT AUBER. Leipsig: Johann Ambrosius Barth. S. v + 234. Preis 6.6.

This is a very well documented experimental study of the fundamental factors entering into the appreciation and interpretation of music. The author acknowledges his debt to Külpe on the experimental side and to Husserl and Bechers on the side of his general theory of knowledge. He deals in detail with the characteristics of musical expressiveness both on their objective side, as they may appear to belong to the musical structure itself or as they are to be found in some kind of definite cognitive reference, and on their subjective side as they rest upon actual experiences of the listeners, or come to a peculiar expression in them. In his final, theoretical, section the author essays the difficult task of analysing the essential content of musical appreciation and its implication, and in particular tries to say explicitly what is involved in *die Ähnlicheitsbeziehung*, a character which music possesses to so marked a degree that we may truly say that "Die musikalische Ausdruckfunktion 'ruht' auf Ähnlicheiten." These final sections are by no means easy to follow.

Naturphilosophische Vorlesungen: über die Grundprobleme des Bewusstseins und des Lebens. Von Prof. Dr MELCHIOR PALÁGYI. Leipsig: Johann Ambrosius Barth. 1924. S. xv + 302. Preis 10.50.

In these twenty lectures the author endeavours to set forth his complete philosophy of life, and theory of knowledge. The emphasis throughout is upon theoretical and general considerations, and though, particularly in the later lectures, there is much discussion about consciousness and conscious activity, the treatment is not profoundly interesting from the point of view of the empirical psychologist. At the same time Dr Pelágyi shows a wide acquaintance with psychological writings.

George Berkeley: Leben und Lehre. Von RUDOLF METZ. Stuttgart: Fr. Frommanns Verlag (H. Kurtz). 1925. S. xi + 248. Preis 6.

Wilhelm Wundt und seine Zeit. Von PETER PETERSEN. Stuttgart: Fr. Frommanns Verlag (H. Kurtz). 1925. S. x + 306. Preis 7.

Both of these are very excellently produced and readable monographs. In each case the general arrangement is first to give an account of the life of the thinker dealt with, and next to expound his views in a more or less critical manner. In the case of the volume on Berkeley an excellent bibliography of works of reference is added. The monographs are produced rather from a philosophical than from a psychological point of view, but they very admirably fulfil their purpose.

Fechner und Lotze. Von MAX WENKSCHER. München: Ernst Reinhardt. 1925. S. 207. Preis M. 4.

This is the thirty-sixth volume of a series entitled *Geschichte der Philosophie in Einzeldarstellungen.* It contains a very straightforward and workmanlike account of Fechner and Lotze, though more attention is paid to their philosophical than to their psychological interests. In each case a brief life is followed by an account of the general development of thought in the writer who is being dealt with, and there is a critical summary of his position at the close. A short and incomplete bibliography of works dealing with Fechner and Lotze is added.

Die Grundgedanken der Machschen Philosophie. Von Dr HUGO DINGLER. Leipsig: Johann Ambrosius Barth. 1924. S. 107. Preis G.M. 3– .

On the whole Mach's contributions to philosophy and psychology are neither too difficult nor too voluminous for the ordinary reader to consult them directly. But if an exposition of the underlying motives and ideas of his view is required, this is both good and short. An admirable portrait is included, and the extracts from Mach's journals, now published for the first time, are of considerable interest.

Bericht über den ersten Kongress für Heilpädagogik in München. 2–5 August, 1922. Herausgegeben von HANS GOEPFERT. Berlin: Julius Springer. 1923. S. 134.

This contains papers and abstracts of papers presented at the above congress. Many of these are of considerable educational interest, and not only discuss various theoretical problems, but give information concerning the actual practice of school hygiene in certain German schools.

Triebgemässer Erlebnisunterricht: Ein Beitrag zur Praxis der Heilpädagogik und der Arbeitsschule. Von Schuldirektor REINHOLD GÜRTLER. Halle a. S. 1924. Carl Marhold. S. 224. Preis G.M. 5.

After a brief and curious plunge into physiology this book proceeds to give a lively and extremely interesting account of its author's methods of training the concrete interests and activities of school children. These are ingenious in themselves and are very well expounded, although perhaps it is possible to over-emphasise the necessity of doing things as a part of school work. However, the author's treatment is to be cordially recommended, and his enthusiasm is splendid.

The Study of Living Things. By E. S. RUSSELL, M.A., D.Sc., F.Z.S. London: Methuen. 1924. Price 5*s.* net.

An attempt is made in this volume to formulate a biological method, which will avoid the inadequacies both of materialism, with its epiphenomenal consciousness, and of vitalism, with its dualism of material and non-material factors. The functional or psycho-biological method here advocated treats the organism as a unity not susceptible of analysis on materialistic or vitalistic lines. The psychological study of behaviour becomes a part of biology, while the physico-chemical study of organic processes lies outside biology, the study of living things as such.

The Contact between Minds. By C. DELISLE BURNS. London: Macmillan. 1923. Price 7*s.* 6*d.* net.

An interesting metaphysical hypothesis is developed by Mr Delisle Burns in this volume. It is suggested that the knowledge of other minds is not merely an inference from the observation of bodily behaviour, but results from an immediate contact between minds by means of a joint or collective 'enjoyment.' "Enjoyment is defined as that experience which distinguishes awareness of the mental process from awareness of an object in or through that process." 'Enjoyment' is not an awareness of the same kind as awareness of an object, and is, therefore, not introspection as generally understood. Nor is it a 'living through' of a mental process—it is not the existence or part of the existence of the mental process, though the term 'enjoyment' sometimes has that meaning in ordinary language. 'Enjoyment' is 'having' a mental process and has much the same meaning as that given to the term by Alexander. It is further maintained that 'enjoyment' is a reverse relation of contemplation. Unlike 'contemplation' which is 'mine' or 'yours,' 'enjoyment' may be 'ours,' and genetically it is probable that 'joint-enjoyment' is the primitive type of experience, and individuality may be said in a sense to develop out of minds-in-communication. Ultimate values (truth, goodness, beauty) can be objects only of minds-in-communication, and traditional genetic psychology may have to reverse the order of development in so far as the grounds of discrimination of value precede the grounds of discrimination of sense-objects. An ingenious theory, it may appeal to the psychologist, though the argument is purely metaphysical and frequently obscure, at least to the non-metaphysician.

The Psychology of Religion. By W. B. SELBIE, M.A., D.D. Oxford: at the Clarendon Press. 1924. Pp. xii + 310. Price 12*s.* 6*d.* net.

Dr Selbie's volume adds nothing of importance to the study of the psychology of religion, and in general reads too much like a departmental defence against a metapsychological relegation of religion to pure subjectivism. There is need for exposition of the psychology of religion from the point of view of the religious expert who is also a psychologist, but it must be a psychological investigation and interpretation, and not, as this book tends to be, a mere review of opinions with the purpose of showing that psychology is not competent to determine the objective truth of religion. There are an unusual number of errors in the spelling of names, and one serious mistake in a quotation from Thouless, on p. 223, where *power* wrongly appears for *person.* MacCurdy and McCurdy, Drever and Dreyer appear as four persons in the Index. They are two: MacCurdy and Drever.

The Concept of Evolution. The Herbert Spencer Lecture, 1924. By H. W. B. JOSEPH. Oxford: at the Clarendon Press. Pp. 32. Price 2*s.* net.

The lecture is another attempt to show that no mechanical explanation of biological and particularly of psychological processes is possible. It is clear, vigorous and well expressed, and will no doubt seem convincing to all who are willing to be convinced.

A Present-Day Conception of Mental Disorders. Harvard Health Talks, 12. By Dr CHARLES MACFIE CAMPBELL. Harvard University Press: Massachusetts. 1924. [English publisher: Mr Humphrey Milford, Oxford University Press.] Price 4*s.* 6*d.* net.

This is a most admirable, clear and untechnical lecture about very common psychological disorders. It could be recommended with the greatest confidence to the general reader who wishes to know something about the causation and character of mental troubles.

The Hygiene of Marriage. By ISOBEL EMSLIE HUTTON, M.D. London: William Heinemann. 1923. Pp. xii + 112. Price 6*s.*

This is a thoroughly sensible, well-written and excellent little book. It is wholly free from the unnecessary sentimentalism which often distinguishes books on this subject, and presents a great deal of valuable and important information in a clear and level-headed manner. It is true that the psychological aspects of the hygiene of marriage are given scant attention, but, after all, too little is known about these for them to figure largely in a book of this scope.

Contraception: its Theory, History and Practice. By MARIE CARMICHAEL STOPES, D.SC., PH.D. London: John Bale, Sons and Danielson, Ltd. 1923. Pp. xxiii + 420. Price 12*s.* 6*d.* net.

This book is by now so well known that there is no need to describe its contents in detail. The theories and methods of birth-control are dealt with fully from a historical, a medical and a legal point of view. On the whole Dr Stopes has performed a difficult task remarkably well. She never hesitates to take a definite side on every question discussed, and if occasionally her treatment gives the impression that a debatable point is more finally settled than is actually the case, at least she is always ready with reasons for the view which she holds. She is alive to the psychological aspects of birth-control and of the different methods by which it has been sought, but it is obvious that much further knowledge is needed on this very important subject. In general on this matter it is the somewhat abnormal cases that most frequently come under observation, and this may easily produce biassed views.

The Fundamentals of Psychology. By W. B. PILLSBURY. New York: The Macmillan Company. 1923. Pp. xiv + 589.

This volume is a reprint of the second edition, which was published in September, 1922. The first edition appeared in 1916. In the new edition the earlier work has been revised and in part recast, with the intention of removing difficulties students have actually experienced, while the subject-matter has been brought up to date without venturing too much into controversial questions. New chapters have been added on "The original nature of man, and the means of modifying behaviour," "Imagination and dreams," and "Work, fatigue and sleep." The book is intended as an introduction to the study of psychology, and fulfills its purpose well.

NOTES ON RECENT PERIODICALS

AMERICAN JOURNAL OF PSYCHOLOGY.

Vol. xxxiv, No. 1.

The Auditory Memory After-Image. (C. C. Dimmick.)

As a result of her experimental enquiry the writer classifies memory-after-images between the after-image and the memory-image, nearer the latter in its individual variations, but with the former in its many sensory characteristics. Investigation was carried out on the auditory memory-after-image, which showed no blending of memory- and after-image, in contradiction to Martin's analysis.

A Peculiar Transformation of Personality Due to Encephalitis Lethargica. (D. McNeil.)

It is suggested that a paralysis of inhibitions, due to disturbance of the perfect functioning of elaborate cerebral mechanisms necessary for control, may explain both the increase of motor activity noted, and the transformation of many character traits.

Some Religious Cults in Jamaica. (M. W. Beckwith.)

A Study of the Müller-Lyer Illusion with Special Reference to Paradoxical Movement and Effect of Attitude. (M. Bates.)

The first part of the series of experiments was an introspective study directed to the content-aspect of the visual experience, with the idea of carrying further Koffka's and Kenkel's work, and the second dealt with the relative effects of the total impression and the analytical attitude towards it. The latter showed objective results from changes of instruction and of stimulus to be similar, indicating like psychological mechanisms. The general conclusion drawn is that the illusion does not lie merely in an inadequate statement of the problems; displacements and movements are not inferred but seen; the illusion cannot occur between the primary mental datum and the judgment, but involves an alteration of datum, and is therefore not 'psychological.' "It exists as a pre-sensational and post-excitatory confluence of the central locus, dependent upon both stimulus and observational attitude for its realisation."

An Experimental Study of Auditory Tied-Images. (F. S. Dimmick.)

This term, first used by Titchener, implies the filling in of incomplete auditory perceptions, and experiments showed their frequency, and their kinship to corresponding sensations differing from them mainly perceptively.

An Experimental Study of the Perception of Stickiness. (M. J. Zigler.)

Analysis and synthesis of the above showed a sustained light pressure in the tactual perception with characteristic increase in clearness and in intensity, with decreased pressure and rather sudden decrease in intensity, a deeper-seated pull as well as superficial light contacts, the perception being aroused by a moving stimulus only.

Some New Apparatus. (K. M. Dallenbach.)

I. A modification of Benussi's Kinohaft. II. A Thermal Stimulator. III. An Apparatus for the Study of Conditions of Clearness.

Vol. xxxiv. Nos. 2 and 3.

A Case of Colour-Blindness to Yellow and to Blue. (F. R. Robinson.)

An unusual case with no shortening of spectrum, sensitivity to red, none to yellow or blue as colour, and weak to green; keen brightness discrimination. There was a congenital basis and apparent degeneration.

Some Problems of Meaning. (R. H. Wheeler.)

The author finds that when analysed down to the level of their component contents, meanings have disappeared and need no structural differentiation; hence, while meanings are to be described in terms of structure, they are to be identified not by contents but by modes of behaviour. Thus the important problems of meaning are functional, and are best elucidated by behaviourist treatment.

The Equation of the Learning Function. (R. M. Ogden.)

In disagreement with L. L. Thurstone with respect to his choice of variables and constants and to the importance ascribed by him to speed in working, the author arrives at two different equations for the learning function in (1) its primary, (2) its secondary form.

The Phenomenon of Meaning. (R. M. Ogden.)

Under the influence of 'Gestalt'-psychology the writer turns to examine his previous work on the consciousness of meaning, taking up anew the problem of imageless thoughts, which may be non-qualitative members of a larger phenomenal construction. In the 'meaning' of a word he distinguishes a complex matrix of 'feeling' primarily sentient, and its configuration, capable, in principle, of reduction to attributive terms.

The Consciousness of Relations. (F. R. Bichowsky.)

A detailed experimental enquiry into this subject failed to reveal any new psychic entity corresponding to relation—"consciousness of two terms in relation involves the existence of a relation between two psychic entities (images, sensations, etc.). The psychic, and presumably the physiological, correlate of the known relation is a relation."

The Effect of Intensity of Illumination on the Acuity of the Normal Eye and Eyes slightly Defective as to Refraction. (C. E. Ferree and G. Rand.)

Experiments showed that defects of refraction are more noticeable in lower than in higher illuminations, and hence require more light for comfortable working than in normal cases; also that in testing acuity, illumination should be varied according to purpose of test; data are given for determination of this.

Auditory and Tactual Perception: The Rôle of the Image. (A. S. Rogers.)

Experiments on the above showed that the percentage of frequency and the complexity of the visual, auditory and tactual images mostly involved depends on the nature of the stimulus, the type on the function of the perception, and the clearness on both type of image and function of perception.

Observations on Taking Peyote (Anhalonium Lewinn). (S. W. Fernberger.)

An experiment with the object of discovering whether the resulting visual manifestations are imaginal in character, and in this respect unsuccessful.

Ernest Meumann 1862–1915. (G. Stoerring.)

William Halse Rivers Rivers 1864–1922. (F. C. Bartlett.)

Calibration of the Galton Whistle. (C. A. Ruckmick.)

An account of different methods of calibration and other improvements adopted by the Edelmann Institute, with gain in reliability.

Position v. *Intensity as a Determinant of Clearness.* (K. M. Dallenbach.)

Experiments show the importance of the former, the most favourable position being to the left and below the point of fixation.

(From the Psychological Laboratory of Cornell University.)

The Phenomenological Description of Musical Intervals. (E. M. Edmonds and M. E. Smith.)

Experiments showed possibility of an absolute characterisation of a bitonal process.

Thunberg's Illusion. (I. Bershanky.)

This occurs equally on forehead, forearm, leg and back, and is conditioned by after-sensations of pressure and cold, which always constitute psychologically the perception of wetness.

On the Estimation of Middle Lines. (H. K. Wolfe.)

A very careful and wide series of experiments with much regularity of results showed among other points of interest that Weber's law as to the constancy of proportion of just perceptible difference in the length of two lines is probably valid for eye measurements of the middle of lines not longer than a metre, and that many conclusions of other writers on this subject are based on insufficient evidence. The value of the statistical method as supplementing exhaustive investigation of a few individuals is emphasised.

An Experimental Study of Memory Colour and Related Phenomena. (G. K. Adams.)

Starting with observations on 'memory colour,' here called 'overlay,' made by Hering and Katz, and, in connection with tones and odours, by Stumpf and Henning respectively, the writer first classifies casual instances and then describes a series of experiments on the five natural objects, snow, coal, gold, blood and grass, mentioned by Hering and Katz as being especially falsified by memory colour; these were followed by controlled experiments on overlays in perceptions of smell, tone, taste and smell, weight, pain, touch, colour. The object was to examine the overlay itself. Its conditions were found to be (1) the object consciousness, (2) an uncritical or casual attitude towards the transformed character of the perception, (3) familiarity with the perception. The destructive effect of the critical attitude necessary for equating perceptions such as those of colour, with the partial exception of taste and smell where familiarity is strong, the facts that one impressive experience with an object can make it 'familiar,' that the closer the similarity between the overlay and the untransformed experience the greater the probability of appearance of the overlay, emerged from the experiments; the last of these gave quantitative measures of strikingly different overlays transforming the same presented object, and revealed the fact that "in its ultimate psychological constitution the memory colour proper is a film."

The Principles and Technique of Mental Measurement. (T. L. Kelly.)

A criticism of the negative conclusions in two articles by E. C. Boring and specification of four units of value in mental measurement, with description of the methods of their determination: (*a*) the sensed difference unit; (*b*) the variability in performance unit; (*c*) the group variability unit; (*d*) the unit resulting in the simplest picture of interrelationships.

The Psychology of Feeling or Affective Reactions. (J. R. Kantor.)

Describing feeling phenomena as actual ways in which a person adapts himself to his affective stimuli, constituting changes and effects on the person himself and not in or upon the stimuli, the writer draws a distinction between these and superficially similar emotional—desire and pain—reactions. After dealing with varieties of stimuli, which do not include composite objects, and typical affective behaviour comprising reflex, habit and volitional reactions among many varieties of cultural conduct, a selection is made of ten classes of the more familiar types in lieu of general classification.

Studies from the Psychological Laboratory of Vassar College.

Absolute Judgments of Character Traits in Self and Others. (M. F. Washburn and V. Stepanova.)

Those referring to no very unfavourable traits tended to similarity in judgments of self and judgments of others, and those referring to positive traits to show underestimation of self as compared with judgments of others on self.

A Study of Revived Emotions. (M. F. Washburn, R. Field and E. D. Wolf.)

Simple experiments showed that (1) the more remote in time the original the less intense is the revival, but with no rapid falling off, (2) this decrease is most rapid with anger, least rapid with joy, (3) joy is most intensely revived.

The Correlation of a Test of Control of Visual Imagery with Estimated Geometrical Ability. (M. F. Washburn, E. Hatt and E. B. Holt.)

A good correlation was obtained of the test devised by the senior of the three authors, and described in Vol. xxv of the Journal.

Affective Sensitiveness in Poets and in Scientific Students. (M. F. Washburn, E. Hatt and E. B. Holt.)

Shown to be higher in the former than in the latter both for articulate sounds and for colour combinations.

Minor Studies from the Psychological Laboratory of Yale University.

The Effects of Practice without Knowledge of Results. (L. T. Spencer.)

A criticism supported by experimental results of Judd's denial of more than a very slight degree of the above.

Minor Studies from the Psychological Laboratory of Clark University.

A Preliminary Study of the Psychology of Mathematical Adjustment. (M. A. Tinker.)

In adjusting quantitative data to some best-fitting mathematical function, the criteria of 'best-fitting' and of 'deviation' have to be established; but with practice, etc. observers can make nearly as accurate linear adjustment of small points as can be measured by the method of least squares. The equation of the sums of positive and negative unsquared deviations seems important in the psychology of adjustment.

Attention as a Condition of Affection. (M. Meenes.)

Experiments carried out with paired comparisons of colours showed the conditioning of affective judgment predominantly by the colours attended to, with but slight effect from the colours not directly attended to.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 11, 1924.	The New Psychology of 'Shape,' by Professor C. Spearman.
	Some factors of Effectiveness in Mental ('Intelligence') Tests, by S. A. Hamid.
February 21, 1925.	Motives in the Acquisition of Skill, by Miss Isabel Burnett.
	On Forgetting the Unpleasant: An Examination into Recent Criticisms of Psycho-Analysis, by Professor T. H. Pear.
March 21, 1925.	The Physics of the Psycho-Galvanic Reflex Phenomenon, by Dr R. H. Thouless.
	The 'Determining Tendency' and Conation, by Rev. R. C. McCarthy.
	A Note on the Psycho-Galvanic Reactions of Anaesthetised Cats, by Dr F. Aveling.

SECTIONAL MEETINGS.

(a) Aesthetics Section.

March 10, 1925.	Extroversion and Introversion from the Artist's Point of View, by A. Thornton.
March 31, 1925.	Children's Drawings, by Miss M. Richardson.

(b) Education Section.

February 9, 1925.	Performance Tests of Intelligence, by Miss Frances Gaw.
March 9, 1925.	The Teaching of Psychology in Training Colleges, by Miss N. G. R. Taylor.

(c) Medical Section.

January 28, 1925.	Substitution, by Dr M. D. Eder.
February 25, 1925.	Some aspects of the Interrelation between Bodily and Mental Disease, by Dr N. H. M. Burke.

LIST OF MEETINGS OF THE SCOTTISH BRANCH OF THE BRITISH PSYCHOLOGICAL SOCIETY

December 1, 1923.	In George Combe Psychological Laboratory, Edinburgh. McDougall's Modifications of his Instinct-Emotion Theories, by Dr James Drever.
	A Method of accurately locating Cold and Heat spots in a Cutaneous Area in a Prolonged Investigation, by Dr David Slight.
February 2, 1924.	In George Combe Psychological Laboratory, Edinburgh. The Significance of the Idea of Death in the Neurotic Mind, by Dr E. H. Connell.
March 1, 1924.	In Physiology Department, University, Glasgow. The Functions of Intervals in Melodies, by Dr H. J. Watt.
November 1, 1924.	In the George Combe Psychological Laboratory, Edinburgh. Intersexuality, by Dr F. A. E. Crew.
December 13, 1924.	In Physiology Department, University, Glasgow. Notes on Character and Temperament Tests, by Dr Mary Collins.
	Methods of Investigating the Colour Sensitivity of Infants, by Dr Shepherd Dawson.

COMMITTEE FOR RESEARCH IN EDUCATION

With a view to facilitating the general co-ordination of educational research, the Committee has made its annual enquiry as to researches in progress (or planned) at the various University Psychological Laboratories and Education Departments. The following is a summary of the replies received:

Researches in progress or planned, December, 1924.

Bedford College, University of London.

"Eidetic Imagery among Children and Students."

King's College, University of London.

"The Study of Character Qualities in Children."

University College, University of London.

1 Manual dexterity.
2. Imagination.
3. Construction and application of tests.
4. Noegenetic processes in tests of ability.
5. The Intelligence needed for Teaching.
6. Impressionistic Estimates of Intelligence.
7. Pictorial and Linguistic Tests.
8. Oscillations of Mental Power.
9. The Work Curve.
10. Influence of Will upon Success at Tests.
11. The Causes of making Errors.
12. Tests of Temperament.
13. Bi-lingual Education.
14. Discrepancy between Tests and Estimates.
15. Spacing in Memorisation.
16. Factors of Efficiency in Tests.
17. Sectioning in Memorisation.
18. Development of correlate-finding in the Child.
19. Practical Intelligence.

George Combe Psychological Laboratory, University of Edinburgh.

1. The Upper Limit of Intelligence.
2. Tests Linguistic and Non-linguistic for the Qualifying Stage.
3. An Investigation into Tests of Character and Temperament.
4. Critical Study of the Principles and Practice of Montessori Methods.

University of Durham.

1. Following up the Careers of Northumberland Children who were tested in 1921 and 1922, and inquiry into their ancestry.
2. Continuance of the Investigation into the Influence of Suggestion in Teaching.
3. Standardisation of further Tests of Intelligence for eleven-year-old Children.
4. Theoretical Investigations into the Application of the Methods of Correlation to Psychological and Educational Questions.
5. Experiments on the Connection of Thought and Speech.
6. Growth of Children's Ideas of Conduct.

University of Glasgow.

"Intelligence of Sick Children in Hospital, in connection with an Investigation under the Medical Research Council."

Jordanhill Training College, Glasgow.

"Study of the Super-Normal Child."

University of Sheffield.

1. The influence of suggestion in solving Arithmetical Problems.
2. A Study of Children's Vocabularies.
3. A Partial Survey of Elementary Schools in Sheffield by means of an Achievement Test in the Fundamental Subjects.
4. An Endeavour to trace the Development of Reasoning Ability by the Application of Standard Reasoning Tests.
5. Vocational Guidance with Special Reference to the Industries and Occupations of Sheffield.
6. Problems of Secondary School Organisation in the light of Group Psychology.

University of Bristol.

1. The Transference of Fatigue.
2. The Relation between Ability and Improvability in certain Mental Functions.
3. The Diagnosis of Mathematical Ability.
4. The Correlations of Ability in Teaching: their significance for Pedagogical Prognosis.
5. The Effect of the Speed Factor in Mental Tests.
6. Study of some Cases of Acquired Retardation in School.
7. As to the Application of Ayre's Spelling Test in Bristol Elementary Schools.
8. As to the Use and Significance of Group Intelligence Tests in Infant and Junior Classes, and the Effect upon Test Results of different Systems of Teaching.
9. As to the Significance of various Tests in Elementary and Secondary Schools.

University of Manchester.

1. The Psycho-galvanic Reflex with particular reference to the cause of changes in resistance in the subjects.
2. An Experimental Examination of Perceptual Grouping.
3. The Influence of Rest and Change upon Industrial Activity.
4. Rest Pauses in Repetition Work.
5. Certain Methods of Advertising.
6. An Examination of the Problems of Muscular Skill, with reference to the correlation existing between the Results of Various Tests, and the possibilities of eventually studying the relation between Muscular Skill and Intelligence.
7. The Rhythm Factor in Literary Appreciation.

University of Leeds.

1. Effects of Games on School Work.
2. Tests of Mathematical Ability.
3. Vocational Tests in Science.
4. Effects of Coaching in Intelligence Tests.
5. Practical Use of Intelligence Tests in Secondary Schools.
6. Vocational Tests in Music.
7. The Psychogalvanic Reflex.

National Institute of Industrial Psychology.

The Research Work of the Institute. Most of the work of the Vocational Section of the Institute is of interest and importance to teachers and educationists generally. Of especial importance at the moment is the research into the Vocational Guidance of young people of 14 years of age. It is being conducted with the approval of the Labour Ministry and the London Education Authority in a representative London area. Its aim is chiefly to give helpful guidance in choosing a vocation to boys and girls leaving the elementary schools at 14 years of age, such advice being determined by psychological as well as social and economic factors. The research work involved includes a psychological examination of each child advised, consultation with parents, teachers and care committees, and co-operation with Juvenile Employment Bureaux and employers in the following up of each child during the first few years of his working life. Incidentally it is hoped that the research, besides leading to standardised tests suitable for work of this kind, will throw light upon many problems which are of interest and concern to teachers and administrators.

In another department, viz. that of Selection, the Institute is doing work of considerable interest. Tests which are devised primarily for the selection of workers for a particular trade or factory are not of course of universal application, but the principles involved and the kinds of qualities tested are worthy of study. The problems of Vocational Selection and the problems of Vocational Guidance are not identical, but research in the one is helpful in illuminating the problems of the other. Whenever a child's aptitude for any particular occupation is under consideration, special tests may be expected to make more precise the decisions which have been reached from the broader and more extensive survey made by the tests used in Vocational Guidance.

(MRS) SUSAN ISAACS, *Hon. Sec.*

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